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This Issue, and Others

With this issue, the second replacement in our Editorial Board. The first last year was Spuhler for Washburn; now Richard Beardsley takes over the editorship of book reviews from David Aberle, who has been so important in the expansion of the ANTHROPOLOGIST. Aberle is spending a year at the Ford Foundation's Center for Advanced Study in the Behavioral Sciences at Stanford; Beardsley will maintain our strength at Ann Arbor.

When is a book not a book? Answer: When it is half an article. Willey and Phillips give us 45,000 words as the second part of a piece which in form and intent is an article and which we are glad to publish (pp. 723 *et seq.*), if only to prove that length by itself is no suitable criterion. (But we can indulge our logic only because the authors found money for the pages to print our quota of articles of normal length.)

Whether this is the longest article in the history of the ANTHROPOLOGIST, summer is no time to determine. Our historical excursions get us into trouble anyway. Last December we said that the first article in this journal explicitly on "acculturation" appeared in 1932. We were corrected by Betty Meggers, who points out two earlier occurrences. One is in W. J. McGee's article, "Piratical Acculturation," in which the first president of our Association distinguishes "amicable" acculturation from "piratical" in which "the interchange is largely inimicable and adventitious" (AMERICAN ANTHROPOLOGIST, o.s. 11, No. 8 [1898]:243). The other is in Otis T. Mason's article, "Similarities in Culture," where "acculturation" includes "contact, commerce, borrowing, appropriating, between peoples . . . " (AMERICAN ANTHROPOLOGIST, o.s. 8, No. 2 [1895]:116–17).

Just after the Summit Conference is a good time to report a translation of an article by A. I. Sobchenko on "Africa in Contemporary American Ethnographic Literature," which appeared in *Sovetskaia Etnografia* (publication of the Academy of Sciences of the U.S.S.R. [Moscow, 1954]), No. 3, pp. 143–53. It was translated by Igor Kopytoff for the Program of African Studies, Northwestern University, and sent to us by Melville Herskovits. The author sees American ethnography as an arm of imperialist penetration, beginning in Africa only in the 1920's "in connection with the penetration of American capital into the countries of Africa, which demanded certain knowledge of local conditions, natural resources, ethnic composition, customs of the people, and the like." Our culture area approach is criticized in that, "as in the works of the culture-historical school, a complex of completely different elements is taken and these are given to the reader as special 'cultural characteristics,' belonging only to the peoples of [the particular area]." Further,

(*Continued on page 865*)

Two Nuba Religions: An Essay in Comparison

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I. PROBLEM AND METHOD

THIS study is, methodologically, a sequel to a previous "Essay in Comparison," exemplifying the same approach and a very similar *Problemstellung* (Nadel 1952). It is concerned with two groups in the Nuba Mountains, the *Heiban* and *Otoro*, whose cultures coincide over a wide range while exhibiting certain limited though marked divergences: these will be regarded as constituting a series of "concomitant variations" significantly interrelated and hence permitting the construction of certain explanatory hypotheses.

Since the general ethnography of the two groups has been presented in the writer's monograph on the Nuba tribes a few introductory remarks will be sufficient to provide the context for the present inquiry.

The Heiban and Otoro are racially identical and live in the same environment, being in fact close neighbors with villages and homesteads of very similar design. They speak almost identical languages (as various vernacular terms will show) and have identical economies based on the same types of resources and technology. They intermarry, though not very frequently. With few exceptions their clan and kinship systems are closely alike: their kinship terminologies, for example, show no differences. The same close similarity also characterizes the legal conceptions of the two groups, their simple, nonrealistic art, and their dances and other forms of recreation. As will be seen, numerous common elements also link their religions.

It is in the field of religion that the crucial "variations" occur, accompanied by others in the field of social and political organization; further differences, not so easily summarized, will be introduced later. Our explanatory hypotheses will bear primarily on the types of religious beliefs and practices characterizing the two groups. In describing these the present study fills a gap in ethnographic knowledge, since the Nuba monograph contained no account of Heiban and Otoro religion.

II. CONCEPTION OF THE UNIVERSE

The Deity

The pivotal concept in either group is that of a supreme being. It is pivotal in the sense that all the other beliefs about the nature of the world presuppose, however vaguely, its creation and arrangement by the deity. Equally, the concept of a supreme being is a "final" concept, entirely self-satisfying and inviting no further speculation. God is called *kalo*, which is also the common word for "earth." But he is understood to be in the sky as well, being, in fact, ubiquitous, invisible and unknowable, with neither sex nor any other human likeness ascribed to him. Though God has created everything, the act of creation is tacitly accepted rather than explicitly asserted or specified.

Much the same is true of his guardianship over the universe and his concern with human fate. Thus the conception of the deity has only weak moral implications: though certain forms of supernatural punishment of evildoers come ultimately from God, since he created and still governs the agencies executing the punishment (lightning, certain diseases), their intervention must be solicited or engineered by human action. The deity is, essentially, morally neutral, and the existence of evil in the divinely created universe is not seen as a problem.

It is in keeping with this conception that neither group conceives of a beyond where goodness is rewarded and evil punished. The Heiban formulate this conception more sharply by insisting that punishment, such as it is, takes place in this life; in the next life good persons and evil fare equally.

In the conception of the beyond the two groups differ a little. Both picture it as an exact copy of this life, where men live in houses, keep livestock, farm the land, marry, and so forth. Logically, both groups provide the dead with food for the journey and make other, similarly realistic, preparations. The Heiban are somewhat more realistic in this respect, for example, in burying two sheep with any man of adult age so that he might have bride-price for remarriage in the other world, and in providing him with fire to light his pipe. Also, their beliefs are more detailed and concrete. Thus they have a special name for the beyond—*kwereny*, "in the back"—though they are not sure where it is located, on the earth or in the sky. The Otoro have no such special name, describing the beyond as being somewhere, probably in the sky, "where one can see God." The Heiban maintain that the soul of the dead bores a tiny hole through the earth covering the grave, through which it escapes shortly after the burial. Kinsmen will visit the grave to discover the hole and so obtain proof of the soul's escape, for a soul which remains imprisoned in the grave may harm the living. In the Otoro view the soul simply leaves the dead body and the grave, which event is neither visualized in detail nor thought to involve any risks or dangers. Both groups believe that the souls of dead people may visit the living, sometimes bringing illness to a person or ill luck to a house. But in Otoro this belief rarely crystallizes in concrete fears. Furthermore, only the Heiban have developed it further, into a belief in ancestral powers. For them, the dead show continued concern in the fate of their progeny, which leads to interventions both helpful and (more often) threatening. Thus, in the event of domestic quarrels which require some solemn reconciliation, the Otoro invoke and sacrifice to God, while the Heiban address themselves to (unnamed) ancestors. Equally, the Heiban are always ready to detect the anger of ancestors over, say, a funeral feast negligently or niggardly performed, in various forms of illness or disaster.

The beliefs of the two groups differ even more sharply as regards the intervention of the deity in human life. The Otoro accept the given form of their rituals as an adequate means of communicating with and influencing the deity. The feelings that should go with the rituals are humility and trust: the priest will address the deity as "God of mine" in the same way, it is explained,

as a "child would call his father." In Heiban it is positively stated that the mere observance of the ritual rules is not enough and that God must be stirred out of his apathy by other means as well—through loud and violent pleas, complaints, and even accusations. Thus the typical Otoro ritual is quiet and subdued; when, in extraordinary crises, a more agitated mood breaks through, this is specially commented on and considered an exception. Few Heiban rituals present a similarly quiet aspect, an excited and violent mood being the rule. Indeed, at certain sacrifices, private or public, individuals may loudly abuse God, accusing him of unconcern and weakness. The explanation is readily and uniformly given: "God does not listen to you if you just pray; you must make him angry first, then he will do what you ask, just to show you his power." This attitude the Otoro consider absurd and unintelligible.

The Heiban notion, that mere appeals and offerings to the deity are insufficient, reveals itself also in another aspect of their rituals. In the "great" cultivation ritual, for example, one beats cornstalks violently against the rocks until the stalks are broken, which means that the rains will similarly be "broken"; in another ritual empty gourds, formerly containing beer, are shaken three times in the direction prescribed by ritual, so that they may be filled again after a prosperous harvest; branches placed across the path to the place of sacrifice after the latter is concluded are meant to "bar the way" to storms which might harm the crops; and at all rituals concerned with cultivation the congregation executes certain elaborate gestures which realistically symbolize the precise effects expected of the particular rite—the height to which the plants should grow, the sprouting of the leaves, the filling out of the ears of grain, and so forth. Though the violent mood mentioned before reappears only in the first of these ritual acts, they are all alike in that they add to the sacrifice and invocation manipulations meant to compel and force nature and hence the deity behind it.

Similar symbolic-compulsive acts are not unknown in Otoro. But they occur only in one or two rituals and do not approach either the elaboration or the precise significance of the Heiban observances.

Spirit Beings

Neither group admits the existence of other deities. There are, however, anthropomorphous beings, local genii or spirits called *nyibor*, which live in rocks, trees, or streams. They are invisible to ordinary humans, though I was once told (in Otoro) about a child who had seen the spirits and for a time lived with them. Medicine experts, on the other hand, combine with their other gifts that of being able to see and discover spirits. The spirits can be of either sex and both good and evil. The good spirits do not do anything in particular; the evil ones haunt certain localities, strike people who go there with sickness or sudden death, and occasionally attack human beings without provocation.

The Heiban conception of these spirits is both more bizarre and more sinister than that of the Otoro. The Otoro believe that the spirits look like

ordinary human beings though they are much stronger; the Heiban picture them as being white of skin, with red hair, and one-legged. They also believe that the female spirits entice certain male humans (who are born with "a second head") to join them in the spirit world; and when the humans are tempted—as they often are—to take advantage of this visit by stealing some of the spirit wealth, grain or livestock, the spirits punish them with illness or death.

In addition to the spirits and the souls of the dead, the world contains certain other invisible creatures—the "people from below" (i.e., living inside the earth), who are "good" people, for example, anxious to insure rain for the humans. The Otoro also speak of "people from above," once more good and helpful, though nothing more precise can be said about them.

Body and Soul

The rest of creation consists of inanimate and animate beings, the former including plants and trees, the latter animals and men. There are no theories or speculations about sun, moon, or stars. The difference between animate and inanimate beings lies in the possession or nonpossession of a "shadow soul"—*dgrim*—which is the word for the shadow of men or animals, the shade thrown by objects, trees or plants being differently named. The shadow soul is already present in the embryo in the womb; on death, it is separated from the body and embarks on a new existence in the beyond, the body turning to dust. Illness always attacks the shadow soul, causing it to "grow weak." Though it would seem more logical to attribute the effects of illness to a loosening of the bond between soul and body, neither group has seized upon this idea or finds the assumption of a "weakening" soul (which yet survives the body) self-contradictory. Nor are there decided views on the nature of dreams, which may show the sleeper the shape of other persons, live or dead. It is thought possible that the persons appearing in dreams may be souls seen by a soul, but here speculation rests. It is, however, positively asserted that the *dgrim* both of living and of dead people can be seen, if only by medicine experts in virtue of their uncommon gifts.

The Otoro conceptions of soul and body are somewhat richer and philosophically more ambitious than those of the Heiban. The Otoro define three constituents of the living being: shadow (or "soul"), breath (or "life principle"), and blood (epitomizing the organism as such). For "breath" one also says "heart," using a special word with a metaphoric meaning, not the name for the physical organ. The shadow is of two kinds, "white" and "black." The living person throws both kinds of shadow (as informants were anxious to demonstrate), the dead body only the "white" one, while the "black" shadow, being the soul that survives death, becomes invisible once life goes. The "breath" ceases at death, while the "blood" disappears gradually with the disintegration of the body. Logically, since the Otoro assume that the soul leaves the body immediately upon death, the dead body is at once called a corpse (*kumdi*). The Heiban, less logically, distinguish between the dead body

before interment (called simply "dead person") and after interment (when it becomes a "corpse," *karu*), even though they believe in a period of transition when the soul still clings to the body in the grave.

The Otoro, incidentally, present their ideas about soul and body in the form of a rudimentary genesis, thus: "When creating man God first made blood; then he added breath; finally he gave man his shadow soul." There are two other attempts, as crude and bare, to account for origins—for the origin of rain making and of certain female food taboos. There is no need to quote the stories, which are not very widely known and which, apart from being flimsy, also refer in a highly inaccurate manner to the observances they are meant to explain. These three accounts contain all there is of Otoro cosmology or mythology, and, if the Otoro are poor "myth builders," the Heiban are none at all.

Miraculous Gifts

The beliefs of Heiban and Otoro lack, as we have seen, any conception of abstract, mystic "forces" which exist autonomously in the world and might become subject to human control. Both groups, however, conceive of certain mysterious enhancements of human capacities, both for good and for evil, which are as fortuitous as they are nonnormal (or "supernatural"). They are referred to simply as "possessions" or "things owned," sometimes by owners initially unaware of the gift, and are best described as "miraculous gifts." These may make a man a rival of spirits (though one doomed in the end), or at least an intermediary between spirits and humans, and between the visible and invisible world.

Such is the medicine expert (called *kumay* in Heiban and *miyay* in Otoro), who can diagnose and cure spirit-caused ailments and see the souls of the dead. Though the calling tends to run in kinship lines (paternal or maternal), it is held to be unpredictable, depending neither on heredity nor on acquired skills but only on a "gift" miraculously bestowed, both on men and on women. In Otoro, meaningful dreams announce its possession; in Heiban, it is even more mysterious, the expert-to-be simply "knowing" that he (or she) has the gift. The expert may well be deceived, as would be shown by the failure of his ministrations. But there is no other test of his capabilities, which, as will be seen later, have in fact no empirical foundation.

The Heiban liken the powers of the medicine expert to those of another mysterious and miraculously gifted individual—the "person with two heads." This is the free translation of the vernacular term *kugi kweti lra*, "man of the head," or *lra* ("head") for short, which is usually explained by saying that such persons have "four eyes" or "another pair of eyes in the back of their head." The "second head" is, of course, invisible, and ordinary people do not recognize a *lra*, though one would suspect one in newborn children who cry all the time and beat their bodies with their fists. In this event a medicine expert may be called in, and if the latter confirms the suspicion he will advise that the infant be killed at once, by crushing its skull and hence destroying

the mystic, and unlucky, "double head." But often the *lra* grows up unsuspected and unsuspecting, for his magic power lies not in his visible body but in his shadow soul, which would roam about at night, harming other shadow souls or plundering the wealth of spirits, as already mentioned. Any person suddenly and unexpectedly grown rich is suspected of being a *lra*. Of this, too, the human owner of the gift may be unaware, thinking only that he was exceptionally lucky, until the spirits punish him—with sudden death, with death by lightning, or with a form of paralysis affecting only the left side of his body. But the suspected owner of a *lra* is in no way ill-treated or ostracized by his fellow men; rather, one appreciates the fact that he is the unwilling vessel of an evil power.

The Otoro have a somewhat similar belief in exceptionally gifted individuals, though it is conceived both more vaguely and on the whole less pessimistically. Thus a person may suddenly discover that when he curses someone or wishes him good luck, e.g., on a hunting expedition, his curses and wishes come true. Other men are said to possess a gift called *yraba*, which word again has no other meaning and cannot be illustrated by any would-be concrete analogy such as the Heiban "double head." The owner of *yraba* is credited, primarily, with an entirely beneficial gift, that of easing difficult confinements by spitting on and massaging the mother's womb. The gift is thought to be hereditary in the same sex, but children brought into the world with the help of *yraba* will possess the same powers. Ordinarily friendly, the gift may turn into a threat: if its owner is angered by the parents of a child to-be-born, he may cause it to be born deformed, but once his anger is pacified he would be able to remedy the effects by a simple treatment (once more, by spitting on the afflicted part of the body and massaging it with water and red clay). Also, there exist evil *yraba* which attack newborn infants without provocation and without the "owner's" desire or knowledge; in this evil version the powers of *yraba* may turn even against the owner's own flesh and blood.

We note the close connection between the "miraculous gifts" and afflictions of the body. The discussion of healing practices will bring further evidence to bear on this "ownership" both of ailments and of their cure.

III. RITUAL

General

Like other primitive societies the Heiban and Otoro lack any conception of worship in the abstract, of a self-satisfying and exalting communion with the deity. Rather, all acts of worship are severely pragmatic, revolving upon needs to be satisfied, risks to be reduced, problems to be resolved. Equally, all acts of worship are ritualized, the appeals for supernatural aid being bound to more or less standardized procedures, to appropriate times and places, and to the manipulation of objects. Sometimes, especially in Heiban, the procedures and manipulations become all-important and are credited with autonomous efficacy (as in the "compelling" gestures mentioned before). But ritualization goes together with conscious expressions of appeal. This is evidenced, in both

groups, in the loosely phrased prayers addressed to God (more rarely, to ancestors) which accompany all rituals and never approximate to precise verbal manipulations (i.e., to magic spells). Taking the belief in the efficacy of manipulations (of words, gestures, objects) to indicate a more "magical" orientation, and the belief in the power of spontaneous prayers or appeals one that is more "religious," we may regard the rituals of the two groups as embracing both, if in different degree.

For the sake of brevity we shall here concentrate upon the major and "public" rituals, meaning by this phrase not so much their scale of celebration as the import ascribed to them, i.e., their concern with the needs or problems affecting the community at large rather than the lives of individuals or kin groups.[1] Of the "private" rituals we need only say that they are linked with birth, death, and marriage, with domestic calamities or fortunes, with the conclusion of pacts of friendship or the settling of quarrels, and with similar personal or family matters.[2]

It is difficult to describe a "typical" ritual, since its features vary widely with the occasion. But the following brief account of a cultivation rite in Heiban presents all the important features.

Priest and congregation assemble in the morning at the place of sacrifice, which is marked by a stone cairn serving as altar. First, some newly cut grain is placed on the cairn, being weighed down with a stone. The priest, assisted by the congregation, lifts the sacrificial animal over the cairn, puts it down again, and kills it with a spear. Everybody touches the blood on the spear blade, and then all clasp hands across the cairn. These acts are accompanied by brief prayers spoken by the priest and congregation in chorus, thus:

> God, give us things today,
> Cause the sacrifice to be good today.
> God, give us grain, give us beer, give us children,
> Give us all things good.

After being skinned the animal is cut up, and the meat is boiled with some grain and heaped on a platter. The people sit down in a circle facing the food and, extending their folded hands toward it, repeat the prayer. Then everyone helps himself to the food. After the meal one leaves the bones, the stripped grain stalks, and morsels of the food on the cairn as a final offering to the deity, whereupon the gathering breaks up without further ceremony.

Let me note a few variants. At minor ceremonials there may be no cairn "altar," a simple flat stone serving the same purpose. Animals already killed, other crops, or prepared food and beer may be added to or replace the sacrificial offerings just described. In rituals involving the idea of consecration (of first-fruits, of a newly filled granary) one also burns certain animal fats, incense fashion, over the things to be consecrated.

Normally there is no music or singing. Women and young people are admitted only at minor ceremonials, but the ritual procedure is by no means secret, everybody knowing what takes place. It is on the whole uncomplicated

and loosely formalized, and involves the handling of only a few objects—plain vessels for food and drink, the spear with which to kill the animal, the crude altar, a splinter of wood on which to place the "incense." Rarely is the choice of the sacrificial animal subject to special rules or mystically significant; often it is determined by practical considerations alone. The animal is invariably a common species, domestic or wild, and the conceptions underlying this as all other offerings are once more unsophisticated, the offering being simply regarded as a gift to the deity (or to spirits and the souls of the dead). In calling the sacrificial food a "gift" the people further underline the character of the ritual as an "appeal." The problem involved in this offering of material substances to nonmaterial beings disturbs no one; if the people speculate at all on this question, they answer it simply by saying that it is the "shadow souls" of the sacrificial items and not the items as physical substances which are offered to and accepted by the transcendental beings. As regards the meal with which the congregation concludes the sacrifice, it has in no way the significance of a "communion."

The simplicity and informality of ritual admit of certain exceptions. Various ceremonials in both groups entail rigid prescriptions as to the direction in which one must face during the ritual acts, the number of items to be sacrificed, and the kind of incense to be used. These rules, however, signify nothing that bears on the aim or success of the ritual but only certain social implications of its performance, namely, the clan membership of priest and congregation (Nadel 1947:96–97). Another exception is more significant since it distinguishes sharply the religious attitudes of the two groups. We have mentioned the symbolic gestures which, in Heiban, complicate the ritual (and render it more "magical"). The Heiban similarly surround one of the common ritual objects, the sacrificial spear, with more elaborate ritualization. It is treated as a sacred instrument, hidden away on normal occasions and requiring to be consecrated before it is first used. Also there is only one such spear in each kin group (a feature especially important in private rites), where it usually belongs to the oldest male member. If the sacrifices thus performed appear to be unlucky, the spear will change hands, passing to another man who, one hopes, will handle it more auspiciously. In Otoro, the sacrificial spear is neither as sacred nor as rare, and new ones are often taken in use without consecration, e.g., when a kin group grows and scatters. Ordinary spears, even knives, may equally be employed without courting disaster. Let me stress that the Heiban beliefs indicate more than merely a greater preoccupation with ritual detail: they introduce into ritual not only an additional mystic manipulation but also an additional risk—the possible ill luck of the spear's owner.

Cultivation Rites

The public rituals have one of two professed aims—to secure rain or to prosper the crops. Only the latter kind of ritual is also public in the sense that it is performed on a communal scale. But though the rain ceremonial is exe-

cuted in the privacy of the priest's house and by him and his kin alone, the whole community takes cognizance of it and may even have to take steps to insure an adequate performance. Both kinds of ritual, too, exhibit their public character in that they constitute specific offices, held by publicly acknowledged experts—the local "grain priest" or "rain maker."

The Heiban grain priest is in charge of five annual rites (called *lobo*), which accentuate various phases of cultivation. The first rite precedes planting and involves, apart from the usual sacrifice, also the burning of incense over the seed grain. The second rite takes place when the crops are about foot-high (and must be "made to grow"). The third rite coincides roughly with the last weeding (when backward plants must be "helped on"), and the fourth, with the first reaping. The last ceremonial once more involves the incense rite, now performed over the newly threshed and stored harvest. The first three rites take place on or near cultivated land; the fourth, on a group of rocks overlooking the farmlands of the community; the last, in the priest's own house. The first and fourth rituals also serve as signals for the respective agricultural activities, the priest being the first member of the community to undertake them, on his land. The premature planting or harvesting by others is believed to destroy the fertility of the land, and such acts call for special purificatory sacrifices as well as for the imposition of fines on the culprits.

The fourth ritual, called "great" *lobo*, is on a much larger scale than the rest. It concludes with a big feast in the priest's house, attended by everybody and celebrated with singing, dancing, and much drinking of beer. It also marks the end of an important, severe taboo: this is imposed, after the third *lobo*, on all singing, dancing, and on certain boisterous games played by the young. All these forms of enjoyment, and even mere whistling or shouting in the standing grain, are thought to endanger the crops, perhaps (there is no agreement on this) because they might provoke the jealousy of the spirits.

Here another magic threat to the fertility of the land may be mentioned. It lies in any crime (such as rape, homicide, or assault) committed on cultivated land; even a scuffle or exchange of blows which leads to blood dripping on the ground would threaten fertility. The crime and breaches of the peace are thereby aggravated, and the disastrous consequences to the land must be warded off by special sacrifices of purification. One does not think in this connection of spirits and their quasi-human idiosyncrasies; rather, the underlying conception is a firm linkage between the moral universe (which is disturbed by such crimes) and the natural universe (sensitive to all kinds of disturbances).

The priesthood of the *lobo* is hereditary in certain lineages, though the succession is far from rigid. Thus, where there are several sons, any one may be chosen, not for reasons of seniority or other obvious qualities such as cleverness or virility (one grain priest I knew was a eunuch), but in virtue of some mysterious, not readily discoverable gift. This gift might be suspected or guessed by fathers or relatives, but would ultimately prove itself in the success of the rituals; nor are "false starts" unheard of or unexpected. The

grain priest lives the life of an ordinary peasant save for one special taboo: he must not eat in the presence of strangers anything that "grows on the ground"; when his wife carries food to him on the farm, she carefully covers it up so that no one should even see the food.

To turn to the Otoro cultivation rites: there are only two of a public character; the others either are missing or are reduced to "private" ceremonials. Only one of the two public rites is called *lobo*, and corresponds to the "great" *lobo* of Heiban in its combined ritual and festive character. It falls, however, in the middle of the rainy season, not coinciding with any of the Heiban dates, and lacks the significance of a "signal" rite. In some Otoro communities both the local and seasonal significance of the rite have disappeared; here the sacrifice has been joined to the triennial age-grade ceremonial *yajo*, which is celebrated on a tribal scale and has the strong connotation of a festivity demonstrating political solidarity (Nadel 1947:145–46). The second Otoro ceremonial precedes the first planting and also ceremonially opens the new hunting season. It consists of a sacrifice of the newly killed or trapped game and of the incense-burning rite, being also known by that name (*kurregyo*).

Unlike the *lobo*, which is entrusted to the local grain priest, the second ritual is the responsibility of the rain maker. If this combination of religious duties suggests that Otoro priestship is less highly specialized than priestship in Heiban, this is evidenced also by another fact. In order to ascertain the precise date of the first-planting rite, the Heiban grain priest must rely on the knowledge of special calendar experts—old men experienced in observing the rising and setting of the sun but holding no office proper. In Otoro there are no such specialists; rather, the rain maker is himself a calendar expert.

Very little need be said about the Otoro grain priest. He assumes his office exactly as in Heiban, though he stands under no taboos indicating his unusual position or mystic powers. Nor do the Otoro practice any of the seasonal taboos which, in Heiban, are a constant reminder of possible disasters. Nor yet do the Otoro believe in the supernatural threats to fertility entailed in crimes committed on cultivated land. Again they see in the performance of rituals and in the simple appeal of prayers a sufficient guarantee of divine protection.

Rain Ritual

The appointment or election of the rain maker closely resembles that of the grain priest and follows the same rules in both groups. But the character of the office differs sharply. In Otoro the rain maker's special powers come into play only when the seasonal rains fail. In this event he is expected to perform the appropriate rite without waiting for any request on the part of the people. The rite takes place at night and in secrecy. The priest kills a "red" sheep and, invoking the deity, fills two gourds with water, which he throws high up in the air: as the water drops down to the ground so rain will fall on the earth. Yet it is thought possible that the priest might, from personal resentment against someone in the community, refuse to perform the ritual. The morality of such a step is not questioned as such; all one would try to do is to

remedy the situation. If the person who is the cause of the rain maker's anger is known, the people will try to persuade him to make amends; failing in this, they would climb to the top of the mountain or assemble in front of the rain maker's house, wailing and crying "as in a funeral" and begging God for rain (as I have witnessed), whereupon the rain maker would always relent and perform the ritual.

Here it is important to emphasize that, for the Otoro, the rain maker is only an intermediary between the people and God. God himself causes the rain in response to sacrifice and prayer. Thus, if the rain maker were to persist in his refusal, it is thought likely (though no one could visualize such a situation) that the direct approach to God by the crying and wailing congregation would even then secure the rain.

The rain maker and his sons (i.e., his potential successors) observe certain food taboos and special rules of personal cleanliness. The rain maker also officiates at the death of people killed by lightning, when a sacrifice is needed to neutralize the supernatural contagion ascribed to this kind of death. This duty the Otoro rain maker shares with his Heiban counterpart.

The Heiban, unlike the Otoro, expect the rain maker to perform the secret, rain-causing ritual every year, and not only when the rains appear to fail. The beginning of the rainy season in fact depends, in their view, on the regular performance of the rite. Again, they are much more ready to suspect the rain maker of omitting the rite because of some private grievance, and any delay in the rains is at once interpreted in this sense. Though the rain maker is firmly expected to deny any such motive, rumors of this kind will at once spread and gain credence. The countermove now lies not in persuasion but in coercion, in forcing the rain maker, by threats and even bodily attacks, to perform his duty. There is some concrete evidence of rain makers' having left their villages, emigrating to other parts, when the rains were late and these threats likely to materialize. As in Otoro, the rain maker is still an intermediary between humanity and deity, but he is an intermediary who is both more indispensable and more unreliable.

This pessimistic and anxious outlook is further accentuated by the duplication of rain experts in Heiban. In addition to the expert responsible for making rain, there is one responsible for stopping or "holding" the rain. He is expected to do so, normally, when the rainy season draws to an end. But once more it is thought possible that resentment or anger might cause the expert to "hold" the rain out of season, so that the person responsible for his mood must be discovered. But now no threats are used against the expert, even if no culprit can be found. Rather, if this is the case, one assumes that the drought must be "natural" and can be broken by the offices of the rain maker proper.

In this view the rain "maker" is a more sinister figure than the rain "holder," for the former cannot plead "natural" causes, and his denial or apparent lack of motives for revenge is inevitably disbelieved. It is interesting to note that in an adjacent community, culturally identical with Heiban, the roles are reversed, the rain "holder" being regarded as dangerous and on occa-

sion evil, while the rain "maker," though not immune from the human weakness of wishing to vent his anger on others, is on the whole considered a friendly and helpful agent. Thus there is some attempt at balancing good and evil, the friendly and the hostile forces of the universe. In Otoro, where the hostile attitude of the rain maker, apart from being thought less likely, can be neutralized by the direct appeal to the deity, this balance seems unnecessary. Nor is the danger of the season failing traced to two different sources and hence doubled, the notion of a special rain "holder" being simply ridiculed.

IV. HEALING PRACTICES

The logical transition from "ritual" to "healing practices" may be seen in a variety of the latter which employs the typical procedures of ritual and represents, with the other observances provoked by domestic calamities, an instance of "private rites." In a sense the healing rites are the least private of all the "private" rites. For while in all others the ritual procedure is in the hands of ordinary individuals (a family head, a father or husband), the treatment of illness usually involves the services of publicly acknowledged experts. Also, certain illnesses carry a far from private import, being understood to represent supernatural sanctions for offenses against tribal or kin group morality.

In Nuba eyes hardly any form of illness is "natural." Various diseases and ailments are in fact traced to specific mystical causes; others, though they cannot be thus identified, are yet "unnatural" in that they are believed to come from the unexplained presence in the organism of some foreign body such as a stone or splinter of wood. Only in the case of minor complaints, which may not call for treatment at all, or of injuries due to obvious accidents is there no search for a hidden cause. Female sterility and male impotence are classed with the mystically caused disabilities.

When there are doubts as to the type of illness afflicting a person, the medicine expert must diagnose it before applying the appropriate treatment. The diagnosis involves nothing in the way of an examination based on acquired skills or experience but is entirely intuitive. The intuition in turn is taken to be part of the "miraculous gift" which makes the medicine man. Nor does the treatment rest even on crude empirical knowledge, say of medicinal herbs or of some simple form of surgery. The mystically induced diseases are treated by ritual entirely; in the others the treatment is ostensibly technical, aiming at the "extraction" of the foreign body, which usually lodges in the chest or abdomen. This is done by massaging the afflicted part with oil, spittle, or animal blood until the intrusive element remains in the hands of the medicine expert and can be triumphantly displayed. Though this spurious surgery simulates a strictly mundane skill, the miraculous and supernatural source of that skill is never forgotten.

As will be seen, the list of mystically induced ailments is much fuller in Heiban than in Otoro. This does not mean, of course, that the respective complaints are unknown in the latter tribe. But it does mean that the Otoro

are aware of fewer causes of disease and hence reckon with fewer risks and mystic dangers. Conversely, the more extensive and precise anticipation in Heiban of these risks and dangers also entails much greater overt anxiety.

The mystic causes of illness may lie in the direct action of God, in the machination of spirits, in the anger or malevolence of the dead, in sorcery, and in the efficacy of magic "roots." The different treatments corresponding to these different causes are often tried out one after the other or even combined.

The forms of illness caused by God directly are the same in both groups—lunacy and leprosy. Both are incurable; while nothing further is known about the first, leprosy is invariably considered a supernatural punishment for grave kinship offenses, such as incest or neglect of blood revenge. This threat is more sweeping in Heiban than in Otoro, for, while the Otoro think only of true incestuous relations (between siblings or parents and children), in Heiban any illicit sexual relationship between kin is believed to invite this penalty.

Spirit-caused diseases, which are of various kinds, or those caused by the souls of the dead require some conciliatory gift or sacrifice or some rite of exorcism. In the latter, the medicine expert sprinkles animal blood over the patient, his kin, and his dwelling, or drags the branch of a thorny shrub of magic qualities through the house and out to some wasteland or river bed, where it is weighted down with a stone. As has been mentioned, only the Heiban believe in revengeful ancestors, who may inflict illness, impotence, or female sterility.

Sorcery, called ***kudigrinna*** in both groups (lit., "seizing the soul"), is essentially punitive magic, which any man or woman can work against a person by whom he or she has been harmed. Unless the subject of the sorcery is in fact guilty, the magic act has no effect. If effective, it causes swellings on the body and eventual death, unless the culprit repents and remedies the wrong he did. The techniques involved are simple: anything belonging to the person to be bewitched will serve (a piece of clothing, food, grass from the roof, also earth scratched from the footprints), if it is taken, hidden, and finally thrown in the fire. As the victim's possession burns to ashes so his "soul" will disintegrate and vanish. In Heiban one may also take the leaves of a certain tree, dip them in the blood of an animal, and sprinkle the blood in the air; at the same time one calls upon God to "send evil to the person (unnamed) who wrought evil."

If Heiban sorcery is simpler and more widely applicable (since it is unnecessary to possess oneself of the victim's property or even to know his identity), it is also thought more destructive, for everybody is believed to be capable of it, though certain clans are credited with particularly effective powers of this kind. In Otoro these powers are thought to belong to certain individuals only, who need not be aware of it until some chance trial convinces them of their gift.

The only remedy open to the victim is to conciliate the sorcerer and, if there is still time, to make him or her lift the curse. There is no protection against ***kudigrinna*** save one's innocence; nor do kinship bonds, of any degree, offer

immunity. In a case I witnessed in Otoro a husband thought himself bewitched by his wife; in a case in Heiban, a son by his father. Yet *kudigrinna* is talked about quite openly without any attempts at secrecy or, for that matter, show of fear. The Otoro even state explicitly that it is senseless to be afraid of something that cannot be helped and is, in any event, worked in secrecy, unbeknown to the victim. Though the Heiban in similar discussions admit to being afraid, there are no overt indications of anything like an ever-present obsession with this particular anxiety. But the act of sorcery itself is viewed differently in the two groups. The Otoro call this magic self-help unequivocally "evil," and informants would admit only that they might resort to it "very rarely." The Heiban regard it as perfectly normal and even as capable of being improved and made easier. They are now also utilizing the services of itinerant shamans from a neighboring tribe, paying them a goat for each "bewitching" of a victim, known or anonymous. The Otoro, then, import moral inhibitions into their belief in punitive magic; the Heiban turn the magic revenge almost into a business proposition.

The belief in magic "roots" (*towa*) is once more limited to Heiban, where it serves to explain a variety of ailments—sores and boils on the legs, inflammation of the eyes and of the throat, and severe rheumatic pains. In spite of the concrete name, which refers to "real roots in the earth," these are invisible to the layman's eye and entirely mystical in nature. They work automatically, without the intervention of the deity, and in largely unexplained fashion. Only the roots responsible for boils and sores are associated with a tangible entity and observable causes, the roots being those of a special, "evil" kind of grain which causes the affliction when, by chance, the grain (or any of its products, such as beer) touches the human skin. At the same time, the evil quality of the grain is said to come not from the plant as such but from its planter or "owner," who is called the "owner" of the disease as well. The other magic roots are similarly "owned" by certain individuals. The "ownership" is hereditary, both in the paternal and maternal line, though nothing can be said about its origins or beginnings. The owner of the disease is frequently capable of curing it, and a number of these "experts" are widely known. But while the harmful gift is hereditary, the cure must be specially learned. Not everybody is sufficiently "clever" to master the skill, which involves extremely complicated manipulations such as repeated sacrifices of unusual animals, accompanied by elaborate and symbolic passes and gestures. Let us note that the highly "magical" character of these cures also renders them precarious, since the slightest error or lapse of memory would invalidate the whole procedure.

A similar mystic "ownership," though much deadlier in its effects, is attributed to one Heiban clan (*Lgoko*). Without any action on the part of the owners, this magic brings sudden death upon any person attempting to steal from a *Lgoko* man. Now this type of automatic "punitive magic" does not fit into the framework of Heiban beliefs. Indirect evidence in fact suggests that the *Lgoko* represent an immigrant group (though long since absorbed in the Heiban community) from one of the neighboring tribes which possess precisely

such a system of clan-bound magic. A similar anomaly, to be similarly explained, also occurs in Otoro, where two clans are credited with corporately "owning" certain magic powers. But these are entirely beneficial, consisting in the cure of intestinal complaints, rheumatic pains, and of a wasting disease (probably sleeping sickness) by a simple ritual manipulation or by the "laying of hands." In the tribes from which the two Otoro clans appear to have come the gift of curing a particular disease is usually combined with that of inflicting it, much as in the "root" diseases of Heiban. It is possible, therefore, that the Otoro, in incorporating the alien clans, have adopted only their beneficial powers, and the Heiban only the malevolent ones. But even if this assumption were incorrect, the fact remains that Otoro culture permits the persistence of an anomaly which adds to the benevolent aspects of the universe, and Heiban culture the persistence of one that adds to its threats.

V. SUMMARY AND FIRST HYPOTHESIS

It is now possible to summarize the character of the two religions and to contrast the basic attitudes or tendencies visible in them.

In Heiban, the acts of worship are more highly ritualized and lean more heavily on "magical" manipulations and symbolic, or symbolic-compulsive, meanings. The greater concern with details of ritual also means a greater awareness of chances of failure—from technical errors, lapses of memory, and the like. Let me here add that this awareness is often visible in the anxiety with which the congregation watches the ministrations of a young or inexperienced "expert." The Heiban tend to multiply and particularize not only the features of ritual but also the supernatural entities (think of their conception of spirits, of "men with two heads," or of revengeful souls of the dead), but they do so only to increase and specify the mystic threats in the world. Altogether, the Heiban world view is more sinister than that of the Otoro and gives greater play to aggression, real or fancied (for example, in their attitude toward the rain maker). For the Otoro, the universe is fairly ordered by a dispassionate deity, so that relatively few, unexcited observances insure the desired normality of life and nature. In Heiban, one feels the need for repeated ritual interventions (think of the more numerous cultivation rites), for excited protestations, and for the coercion of the deity. Only the Otoro have attempted to rationalize their beliefs in a rudimentary cosmology and mythology.

Attempting a more concise (if oversimplified) summary, we may call the Heiban more magically oriented, more aggressive, emotionally tense, coercive and pessimistic, and the Otoro more "religious," calm, dispassionate, submissive, and optimistic. There is no need to point out that the traits in each group hang together in something like a coherent "syndrome." What is important is that the same cognitive and emotional traits reappear both in other provinces of religion (i.e., in the "private" rites) and in the attitudes or biases visible in mundane life. In other words, they add up to two consistent, contrasted "mentalities" (or "orientations," forms of "ethos," "personalities" —whichever description one may choose) (Nadel 1947:171–74).

To accept this consistent "mentality" is not, however, to dispose of the

problem of explanation. To some extent the syndromes themselves contain explanatory clues, for within each, certain traits appear to be causally interconnected so that the presence of one would explain that of the other. Thus the bias of the Heiban toward "magical" manipulations and compulsive symbolism might well be a consequence of their more sinister world view; and the latter is more likely to be accepted (and perpetuated) by children brought up in an atmosphere of violence and aggression.[3] But we will for the moment disregard these internal, short-range, and essentially circular linkages. We will assume that it is possible to go beyond them and to establish more significant, because more far-reaching, causal relations, connecting religion with social facts of an altogether different order, that is, with conditions which are functionally autonomous and hence represent "independent" variables (upon which the facts to be explained are "dependent"). I am referring here to the social or cultural divergences which, as explained in the beginning, appear together with the divergences in religion. They concern (A) the size and political integration of the two groups; (B) the regulation of adolescence; (C) the jural status of wives; and (D) sexual morality.

The preliminary hypothesis, then, is that one or more of these factors will prove capable of explaining, in causal terms, the difference in the religious orientation (or "mentality") of the two groups.

Verification

A

While the Heiban form a small group, occupying a small, compact territory, the Otoro, more than ten times their size, are spread over a large hill massif. Though the Otoro are still in many ways split up into separate communities, each roughly corresponding to the type of group exemplified by Heiban, an embracing tribal unity has also emerged, helped by the centralized chieftainship now several generations old. Neither development took place in Heiban. But the resulting differences in social organization offer no evidence in support of our hypothesis, rather the contrary. It can be shown that both the wider tribal unity and the centralized chieftainship have created new problems and moral dilemmas, such as conflicts of loyalty and uncertainties about the duty of blood revenge (Nadel 1947:154). Their emergence thus fostered tensions and anxieties rather than lessened them.

B

Only the Otoro have evolved a system of age-grades providing a step-by-step framework for adolescence, both male and female. Offering as it does planned occasions for competition in aggressive sports and games, the age-grade system may be assumed to release as well as canalize violent tendencies and implant the values of self-restraint. It does so, further, by its rules of etiquette and team co-operation, and by its hierarchy of authority. Equally, it lessens adolescent sexual tensions through associating the two sexes in acknowledged and balanced relationships. More generally, the Otoro age-grades

habituate the adolescent to a well-ordered, predictable scheme of roles and relationships in which uncertainties of expectation and anxieties over right and wrong conduct are reduced to a minimum. In Heiban, adolescence is without such marked steps and set relationships, being essentially unregulated. Here, then, the observed covariations demonstrate an intelligible, meaningful, causal nexus, in the sense of our hypothesis.

C

All societies operating with unilinear descent groups must resolve the problem of the ambiguous position of wives, for the wife comes as a stranger to her conjugal kin group yet remains in it not only as a domestic partner but also as the progenitress of the continuing descent line. Otoro and Heiban have in this respect worked out opposite solutions. In Otoro, the wife is completely adopted into her husband's lineage, being jurally and symbolically severed from her natal kin group. In Heiban, the adoption and severance are alike incomplete, wives and mothers belonging to their conjugal kin groups only for life and for the explicit purpose of giving sexual satisfaction to the husband and offspring to his lineage. Detailed evidence to support this interpretation has been given elsewhere (Nadel 1947:107–8, 120, 122, 124). Here I would argue that the "incomplete" adoption of wives entails serious conflicts of loyalty, anxieties over the success of the marriage, and jural ambiguities which must engender tensions both within and between kin groups. Their reality is borne out by the many family quarrels in Heiban, and by the marked concern of the men over the fecundity of the women no less than the sexual satisfaction they provide. These data, too, may be said to support our hypothesis.

D

The Otoro are markedly reserved when discussing sexual matters, and never do so in large company. Only in privacy, between two friends, is there any bragging about sexual adventures. Marital experiences are never discussed, nor are sexual topics normally touched upon in the presence of the other sex. At the same time, the Otoro show no traces of embarrassment, shame, or guilt in this connection. Sexual dreams are considered meaningless, and young men do not feel ashamed to talk about them, even to their girl friends. In Heiban, sexual bragging and joking among the men is strongly pronounced and one's sexual adventures are openly discussed, in provocative fashion, even in the presence of the other sex; also husbands generally complain about the frigidity of their wives and the fact that they "tire easily." Yet a feeling of guilt is equally noticeable, for it is considered shameful to talk about sexual dreams, which are found puzzling and disturbing.

In both groups, finally, male homosexuality occurs. But while in Heiban the homosexuals have no special niche in cultural life, being simply regarded as "abnormal," in Otoro they are allocated a special role, allowed to dress like females and to live in most respects a woman's life.

In certain respects the attitude to sex is accounted for by facts previously

discussed—the habitual association of the sexes in the Otoro age-grades, and the "incomplete" adoption of wives in Heiban. But the Otoro "reserve" in sexual matters, and the Heiban "freedom," could as convincingly be related to "mentalities" which are the exact opposite of those actually observed, on the assumption that "reserve" might connote repression (and hence anxiety and tension), and license, the absence of inhibitions and their psychological consequences. The possibility of constitutional differences in sexuality being unverifiable, we can presume no further causal linkage, only the extension of the two contrasted syndromes now to the sphere of sex morality. The different treatment of sexual deviants, more "orderly" in Otoro and unregulated in Heiban, would similarly fit into the picture.

VI. CONCLUSIONS

It will be noted that the two causal interrelations which proved verifiable have a closely similar import: both refer essentially to the presence or absence of social norms which would increase the set patterns of roles and relationships and decrease ambiguities or uncertainties of conduct. Differently expressed, the causal efficacies here suggested derive from aspects of the social system which reduce or enhance anomy. The more specific, observed interrelations, then, between the character of religion on one side, and regulation of adolescence and jural status of wives on the other, can be generalized in this sense, now predicating the dependence of the religious orientation on the varying pressure of anomy (or its opposite, normativeness). Expressed schematically, and as a balance of opposed tendencies: anomy $\gtrless$ normativeness $\therefore$ religious pessimism (etc.) $\gtrless$ optimism. However imprecise and preliminary this formulation, it indicates (or foreshadows) a general social regularity or "law."

But even our observed specific correlations can only be stated in a form in some measure imprecise and preliminary. For example, we cannot, on the facts at our disposal, decide whether the two particular instances of anomy, unregulated adolescence and ambiguous position of wives, exercise their presumed effects singly or only in combination and to an equal or unequal degree. Nor can we estimate the relevance for this efficacy of all the other features of the two cultures, that is, the features which happen to be identical, satisfying the crucial condition of *ceteris paribus*.

Furthermore, the "far-reaching" causality we postulated is not only incomplete but also as nonsequential.[4] No precise genesis can be suggested whereby the institution (for whatever reason) of age-grades or of the rule of complete adoption precedes in any definable historical sense the appropriate religious development. All that can be assumed is that the social norms provide a continuous causation for the emergence of particular religious conceptions. Nor does the causal nexus account for every characteristic of the two religions; and those not so accounted for (e.g., the concern or unconcern with compulsive symbols) must be taken to be the effects of some secondary causality, implied in the internal, "short-range" linkages mentioned before. Equally, the emergence of the religious conceptions must be understood to entail

additional circular linkages at all stages (such as that between upbringing and the readiness to embrace a particular world view).

Finally, on the grounds of general psychological knowledge, we must admit that the scope of our "law" is likely to be limited. The nexus here assumed presupposes psychological mechanisms whereby tensions and anxieties arising from the normlessness of social life are *projected* into an analogous, "pessimistic" religion. There are, however, two other possibilities. (1) Anxieties and tensions might be engendered not only by the lack of normativeness but by its over-rigidity, which would be felt as frustrating; and (2) the anxieties and tensions might, instead of being projected, be *compensated* by a contrasted, "optimistic," religious orientation. Some such limitations are true of most or all scientific "laws"; in our case it must be left to further investigations to specify the relevant conditions.

Though these qualifications detract not a little from the precision of the causal nexus, they do not affect its validity. When we previously spoke of the causes to be discovered as "independent" variables, possessing "functional autonomy," we were merely stating the logical requirement of any assumption of valid explanatory relations (Nadel 1951:243–44). The social norms here adduced satisfy this condition in that they can be thought of apart from the facts they are meant to explain. More precisely, they are intelligible in their own right, as solutions of given, ubiquitous problems of social existence. But here the causal nexus must end. It is perhaps not meaningless to try to carry it further by asking why the problems of what to do with adolescents and how to fit marriage into a unilinear descent system have been resolved in this contrasted fashion by the two groups. But it is, in the narrow context of this inquiry, unprofitable to do so.

NOTES

[1] The scale of celebration has of course an importance in its own right in that it indicates both the degree to which the religious activities tax the economic resources of the group and the manner in which rituals implicate (and reinforce) social structure. Both these viewpoints will here be disregarded.

[2] Several of these "private" rites have been outlined in my Nuba monograph (1947).

[3] It is further possible that an existing cosmology or mythology, as it attempts to explain and rationalize the universe, also reduces mystic fears and felt uncertainties, while its absence (as in Heiban) would foster a neurotic, pessimistic outlook (see Nadel 1952:27). But the difference in this respect between the two groups seems too slight to warrant any definite inference.

[4] It is, however, a true causality, i.e., nonreversible, for it would make no sense to assume, say, that freedom from anxiety can account for the complete adoption of wives, or a pessimistic world view for adolescence left unregulated.

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Status Rivalry and Cultural Evolution in Polynesia[1]

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INTRODUCTION

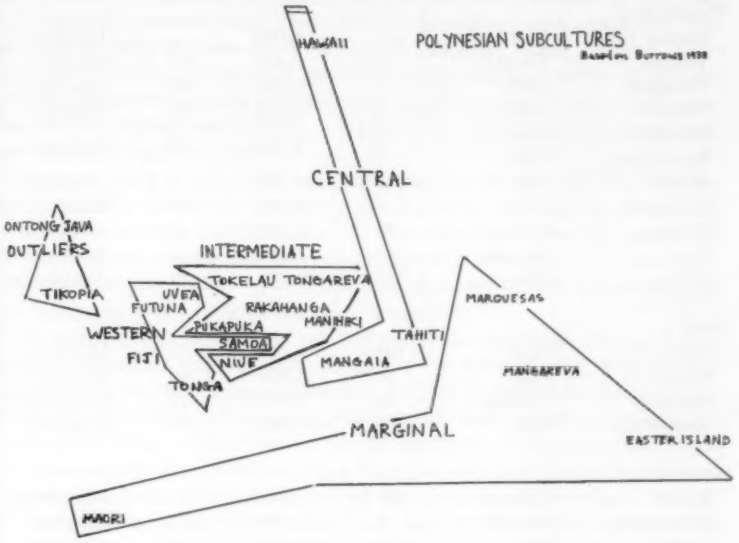
CULTURAL evolution, to rephrase Maitland's classic remark, will be history or nothing. If it is to be history, its proper focus is the culture area, or to be more precise, the comparative study of culture areas. A culture area comprises historically related societies each showing significant variations from a common area pattern. In these variations—their nature, origin, and direction—are revealed the basic processes of cultural development in the area, that is, its cultural evolution. It is from a comparison of culture areas rather than from the comparative study of historically unrelated societies that the more general and more meaningful laws of development may emerge. Boas (1896) was the first to suggest such a comparative procedure as a way to avoid the pitfalls into which the old comparative method had led the nineteenth-century evolutionists.

For a number of reasons, Polynesia is a particularly suitable area for such a comparative study.[2] Since the area was populated rather late, perhaps within the last 3,000 years (Spoehr 1952), its underlying historical unity is still abundantly clear—even linguistic variations are relatively minor (Elbert 1953). At the same time, Polynesian cultures vary in a continuous series from the "simple" atoll societies of Ontong Java and Pukapuka to the highly organized "feudal" kingdoms of Hawaii. These variations suggest an evolutionary sequence, which is borne out by evidence from tribal historical traditions (presumably reliable for comparatively recent events) and from other sources.

The present paper takes as its starting point the dominant values of Polynesian culture, those involving concern with social status. Polynesian society is founded upon social inequality and, despite an aristocratic doctrine of hereditary rank, permits its members to compete for position, for prestige, and for power. In one way or another, then, the history of every Polynesian society has been affected by status rivalry, and under the proper conditions the effects of this rivalry have been felt in every vital center of the culture. Rivalry raises issues and provokes conflicts that can never be fully resolved. It promotes a sequence of culture changes that take their character and direction in part from the momentum of status rivalry itself and in part from the particular physical and cultural setting of each island. Thus, the differing ecologies of atolls and high islands, variations in population density, varieties of subsistence techniques, levels of economic productivity, systems of property relations, the role of migrations and military conquests, diffusion, and, finally, the specific historical "accidents" that occur in wars, migrations, and contests for power—all influence and are in turn influenced by the dominant motive of status rivalry. Many facets of culture, on the other hand, are only barely

touched by status rivalry and lie, therefore, outside the scope of this paper. Data on pertinent physical features are presented in Table 1 and, on diffusion, in the accompanying chart of subculture areas.

From the point of view of status rivalry, Polynesian societies can be ranged in approximate order along a scale that expresses variations in the



character and intensity of the conflict for position and power. Reading in ascending order, from Ontong Java to Hawaii, this scale divides into three historical phases which I call "Traditional," "Open," and "Stratified." The Traditional may be considered to represent early stages of Polynesian cultural development, the Open to be illustrative of transitional conditions, and the Stratified, of the culminating phases. Table 1 shows the postulated sequence of cultural development. The societies at the midpoint of each phase are its prototype representatives, and those at the ends are transitional since each phase shades into the next. Retrogressions may have taken place but were incidental to the main lines of development. The process of development, needless to say, was far more complex than the schematic presentation that follows.

THE TRADITIONAL SOCIETIES

Maori represented the Traditional order at the peak of its development. All Maori tribes claimed descent from common first-immigrant ancestors, and genealogical bonds along with more vital common economic and social interests linked tribe, lineage (*hapu*), and family (*whanau*) into a tight unit.

TABLE 1

POLYNESIAN ECOLOGY AND DEMOGRAPHY

	Physical Character	Sq. Miles	Pop.	Pop./Sq. Mile	Productivity
TRADITIONAL					
Ontong Java	Atoll	n.a.	5,000	...	Marginal
Tikopia	Volcanic	3	1,300	433	Marginal
Pukapuka	Atoll	2	1,500	750	Marginal
Tokelau	Atoll	4	1,200	300	Marginal
Manihiki	Atoll	1.5	1,000	285	Marginal
Rakahanga	Atoll	2			
Maori	High	102,000	100,000	9.8	Abundance
Tongareva	Atoll	6	2,000	333	Marginal
Futuna	Volcanic	25	1,000	40	Abundance
Uvea	Volcanic	23	3,000	130	Abundance
Samoa	High	1,200	50,000	41	Abundance
OPEN					
Niue	Atoll	100	5,000	50	Marginal
Easter	High	50	3,000	60	Marginal
Mangaia	Volcanic	27	3,000	111	Abundance
Marquesas	High	400	100,000	250	Marginal
STRATIFIED					
Tonga	High	269	25,000	93	Abundance
Mangareva	Volcanic	6	2,000	333	Marginal
Tahiti	High	593	100,000	168	Abundance
Hawaii	High	6,435	200,000	32	Abundance

Statistics from Freeman (1951) and from the ethnographies. Productivity is an estimate from the ethnographies. Population figures are not very reliable.

Hereditary rank set the pattern for political authority so that the senior male of one of the senior lineages of the tribe was tribal chief (*ariki*); the senior male in the senior line in each *hapu* was *hapu* chief; and a senior male was head of his *whanau*. As everywhere in Polynesia, *mana* was an aspect of rank. The Maori related *mana* directly to seniority and protected it by *tapus*.They regarded all males and high-born females as in some degree sacred, attributing the highest sacredness to the *ariki*. By vague genealogical criteria junior lines shaded into commoner status whose lowest branches sometimes served their higher-ranking kin as servants and as menials. Slavery was a temporary condition of war captives. Rank defined authority, economic and other privileges, responsibilities, and etiquette. Conferred by birth, it was demeaned by cowardice, low marriages, capture in war, and by other lapses of dignity. Lost rank was lost *mana*, and for that reason alone, as Best has noted, a Maori vigorously resisted affronts to his rank. When, as rarely happened, chiefs were incompetent, they were replaced.

Maori offered only limited social mobility. Other Traditional societies allowed for more. Manu'a (eastern Samoa) had markedly subverted hereditary rank without quite abandoning it. The *Tui Manu'a*, as high chief, and all subordinate chiefs and talking chiefs held hereditary rank in the Great Fono, but adroit and ambitious men advanced their rank and influence by the judicious use of wealth or assumed leadership outside the formal scheme of the *fono*. In Tongareva, more strongly committed to hereditary rank, wars and conquests often reduced rulers to the status of subjects. There, because of the importance of war, land-wealth and leadership—the ingredients of military success—almost rivaled inherited rank as avenues to power. A similar situation prevailed in Futuna and in Uvea. Futuna, Uvea, and Manua, in fact, come close to being Open societies.

Even in such closely knit atoll communities as Manihiki-Rakahanga, lineage rivalries disturbed the orderly succession to chieftainship. Traditions there tell how a dispute between the two wives of an *ariki* over the question of whose son should succeed him was compromised by the creation of a new title, "Land Distributor." The new title-holder then rallied his supporters and eventually two *arikis* divided the rule—unequally.

In several places hereditary rank had barely developed. In Ontong Java a strong man and his followers established the first kingship and, after a series of dynastic wars, one lineage triumphed and set up succession by seniority. Apart from the offices of king and lineage head, rank was of no importance. Rank meant little more in Tikopia and in Pukapuka. Traditions from Pukapuka also suggest that hereditary rank came late and that the first rulers were the old men. Later, four *aliki* contested for supremacy: the winner installed his lineage as the ruling dynasty. But here, too, only the chiefs held rank, the rest of the population being known simply as "the people." In the Tokelaus, age, ability, and material substance were the true measures of status, which was, however, but lightly considered. Only the *matai* ("headman") held office by seniority.

These brief illustrations show the unstable foundations of graded hereditary rank in the Traditional societies. Even those societies, such as the Maori, that had fully incorporated the tradition of graded hereditary rank, broke with it when it conflicted with personal ambition and with differences in ability. These societies retained their traditional character against the Polynesian dictum that fitness supersedes birth only because the tests of fitness were not too severe.

THE OPEN SOCIETIES

Chronic guerrilla warfare posed the most demanding tests of fitness for the aristocracies of the Open societies. As their histories show, the old aristocracies were eventually pushed into the background by warriors and by resourceful political leaders.

Niue had neither aristocracy, priesthood, nor genealogies. The real powers were the warriors (*toa*), who ranked first in the social scale ahead of the *fekafekua*, "servants of the toa," and the *lalo tangata*, "low people," or those who ran away in battle. Niuean traditions, however, describe an initial migration

from Samoa and a later invasion from Tonga that brought in a kingship along Tongan lines. In that case, the Niueans may very well have had an aristocracy that later became inconsequential.

On Easter Island a more virile status scale based on wealth and military prowess overshadowed the traditional scheme. The *ariki mau* was a high priest, while the warriors were the temporal rulers. Tribal traditions speak of a common ancestor who was the discoverer and first king of the island. He apportioned the island among his children, each of whom then founded a tribe. One of the tribes controlled the *ariki* title. In the beginning, Métraux has suggested (1940), the kings were considered to be deities and held absolute power, but in the end the warriors took over, leaving the king only the prestige of his supernatural powers and certain personal privileges.

The Marquesans came closest to maintaining a traditional aristocracy. Yet here, too, achievement scored first. Position and influence went to those who had done most for the tribe. By such criteria the *toa* and the rich stood high. Chieftainship was ordinarily seized by those who had won powerful backing by diplomatic marriages, strategic alliances, and by effective displays of wealth and leadership.

Mangaia illustrates best the transitional character of the Open societies. On that little volcanic island a hereditary aristocracy had been eclipsed by the warriors (*toa*), who commanded the highest prestige, the largest estates, captive vassals, friendly satellites, and because of all these, authority. The hereditary *ariki* held the highest rank, but the effective ruler was the Temporal Lord of Mangaia, chosen from among the greatest warriors. Mangaian traditions tell of efforts to make the Temporal Lordship hereditary. But the conditions that undermined the old aristocracy did not favor its replacement by a new one. The shifting tides of military fortune constantly reshuffled the social order as first one tribe triumphed and then another. At the same time, the urgent requirements of warfare placed a high premium upon fighting skills, upon wealth, and, in general, upon the ability to command followers. The high social mobility perpetuated by these conditions came close to creating a socioeconomic class in place of the older scheme of hereditary rank.

High social mobility was one step toward stratification; the weakening of kinship ties was another. Strong kin ties may be compatible with graded hereditary rank but not with stratification that cuts kinship solidarity at the class line. By the same token, weakened kin ties favor stratification. Traditional kinship governed in Mangaia, but not exclusively. Warfare rearranged descent lines and conditions of residence as men sought safety with their wives' lineage when that lineage was the enemy. Their descendants might then remain with the maternal lineage, return to the paternal fold, or move into still another lineage, depending upon military conditions. By Polynesian standards the lineages became conglomerate, and the traditional patrilocal household became a *mêlange* of immediate family kin, distant kin, war captives and others who served as menials in return for protection. Captives had the choice of tribal citizenship or of defiant servitude. Even as servants they

shared, along with kinsmen, in some of the privileges of their masters. Warfare added one other strain upon kinship bonds: because of frequent changes of family allegiances, close kin met on the battlefield as enemies. In one such instance, a son killed his own father. Strong warriors heading the largest households as lords and protectors became centers of direct political power. Chiefs bolstered their might by adding to their household a body of trusted warriors.

The Open societies then were definitely exploitative—less so than the Stratified, but far more than the Traditional. In Mangaia, the exploited were mainly the outsiders. Exploitation of outsiders, however, may well set a standard for the exploitation of one's own, as seems to have been the case in the Marquesas and in Niue. There, weak and poor families sought the protection of strong chiefs whom they served as subordinates and to whom they paid tribute for grants of land. These societies have been called emergent "feudal" orders (Handy 1923; Loeb 1926). Because land rights remained open to all tribesmen, the Open societies cannot be regarded as stratified. Even so, some Marquesan chiefs had begun to regard tribal lands as their own (Handy 1923).

Why the Open societies stopped just short of stratification is not quite clear. Perhaps the indecisiveness of their wars prevented the consolidation of power and kept the land systems fluid. Military indecisiveness itself was probably due to rugged terrain, food scarcity, and loose political organization. In Mangaia, to return to that example, bands of enemy warriors might resist indefinitely from the shelter of its many caves. Food shortages and limited economic elasticity equalized contending rivals. Even more important may have been their political doctrine of warfare. Because they felt land shortage as their main problem, Mangaian victors failed to organize for their own benefit the crop production of defeated tribes but, instead, dispossessed them from fertile taro lands onto unproductive scrub lands. This may have been the least efficient way to strengthen the victor's economy. Moreover, since their goal was to displace an enemy from his land, there was little basis for a negotiated peace. The enfeebling wars went on.

Stratification seems to have resulted from the consolidation of military gains. So long as the Open societies could not achieve relative political stability, the lines of privilege remained too fluid to facilitate the formation of classes.

THE STRATIFIED SOCIETIES

Tonga, the least stratified of the Stratified societies, held fully to the formal structure of graded hereditary rank but changed its economic content. All Tongans were ranked from the sacred *Tui Tonga* at the top to the lowliest commoner at the bottom, and all social grades were linked by genealogical records that gave commoners the right to assert their kinship with chiefs. The *Tui Tonga*, however, held title to all land and awarded holdings to chiefs according to their rank as well as in proportion to their services to him. The gentry in turn allotted land to their kin on the same principle. Details of the land system, unfortunately, are vague. Thomson (1894) and others cited by Gifford (1929:

170) stressed the sovereign rights of the *Tui Tonga* over land, and Gifford (1929:174) summed up, "A chief . . . could dispossess his commoners at any time, or transfer them from one part of his land to another, even though they opposed the move." Apparently, the land system had driven a wedge between noble and commoner and hence among kinsmen. In some respects, in fact, the commoners were treated as were the alien captives in the Open societies (for examples, see Gifford 1929:127).

The orderly arrangement of landholdings by rank was frequently upset by wars, revolts, and political intrigues. To hold a precarious political balance chiefs rewarded their friends with land and expropriated their rivals (Martin 1817). In this way, the highest-ranking titles sometimes lost much of their holdings and the lower ranks of gentry came to command the largest estates. Hereditary rank continued to rule etiquette, but influence and power came from landownership.

The traditional history of Tonga describes the gradual usurpation of power from the priest-king *Tui Tonga* and the consequent increase in authority of secular rulers, the *Tui Kanokupolu*. As early as the fifteenth century, these traditions say, ambitious lesser chiefs arranged for the assassination of one *Tui Tonga* after another, acts that led to much turmoil and warfare. As a measure of self-defense, the then King Kauulufonua introduced a buffer by naming his son the secular executive with the title *Tui Haa Takalaua* and with supervision of the plantations as one of his main duties. Later (seventeenth century) the *Takalaua* named his son as a land supervisor with the title *Tui Kanokupolu*, and the latter's line eventually became the real powers. The traditions do not deal with changes in the pattern of landholding. However, frequent and prolonged civil wars for succession during which the Tongan islands were divided into many fortified garrisons may very well have paved the way to a "feudalization" of landholdings. The fact that political power finally went to overseers suggests further that control over land had become a major issue in Tonga after the *Tui Tonga*-ship had begun to decline.

The conflict between the claims of hereditary rank and landed wealth raged even more fiercely in Mangareva, the most "open" of the Stratified societies. The theory of a graded hereditary aristocracy with a supreme chief (*aka-ariki*) at its head lay at the heart of the social order. It was a theory, however, honored more in the breach. Dynastic wars inevitably followed the death of a ruler. The victor seized his enemies' lands and gave them to his followers. Hereditary rank never lost its prestige, but the power and influence that wealth conferred were not easily overcome. Mangarevan society became so mobile, in fact, that for a time a line of commoners held the high office of *aka-ariki*.

Increased social mobility and the system of land tenure accented class lines. Eventually, ten aristocratic families came to own all the land, each ruling its territory as a "feudal" domain. Landowners subleased land-shares to kin and supporters, in return for tribute and military services. At the bot-

tom of the social scale the landless "rats" lived as despised and hungry fishermen, and often had to rob graves for food.

Mangarevan traditions are narrations of dynastic wars, fratricidal strife, and interrupted successions, in the course of which power went from the supreme *ariki* to contending powerful chiefs. The strong expropriated the weak, and the vanquished were driven from the land altogether. Mangarevan traditions bear out rather clearly the postulated sequence: Traditional–Open–Stratified.

In Tahiti, hereditary rank was a more stable feature of the social order than in either Mangareva or Tonga. Chiefs (*arii*) and the landed gentry (*raatira*) were graded by seniority and held landed estates proportionate with their rank. They owed tribute and military services to the supreme chief, but their lands were immune from royal seizure. The bulk of the population consisted of the landless *manahune*, so separated socially from the gentry as to be considered by them as an alien group.

Tahitian traditions speak of an earlier more democratic society conquered and reorganized by invaders. Handy (1930) has, therefore, advanced the theory that Tahitian society was a fusion of a simpler indigenous culture with that of a more advanced alien invading culture. Coming presumably from nearby Raiatea in the seventh century, the invaders set themselves up as the rulers, while the *raatira* came into being by intermarriages of followers of the conquerors and the local population. While details of the process are lacking, the suggestion, at least, is that the *raatira* were given land grants in return for military services and that the *manahune* were, accordingly, forcibly expropriated.

Hawaii carried forward all the features of the Stratified societies and, at the same time, resolved the conflict between centralized and decentralized authority by granting to the supreme chief (*moi*) title to all land in his domain. This differed from the rights of the *Tui Tonga* in that each new Hawaiian administration systematically expropriated its opponents, rewarded its supporters, and curtailed the influence of potential rivals by giving them estates smaller than due their rank. By this "spoils system" the Hawaiian rulers frankly acknowledged the equation: land-wealth equals power. The spoils system promoted social mobility among the upper ranks and left the lower ranks least disturbed. As in all the Stratified societies, land relations were "feudal." The gentry held land in return for tribute and services, and the commoners (*makaainana*) were the landless plantation workers who could be—although they rarely were—evicted. An outcaste group, the despised *kauwa*, were totally removed from the land.

Hawaiian traditions describe close relations with Tahiti; in fact, Buck (in Handy 1933) believes that the Hawaiian nobility came from the Society Islands to impose their rule upon the natives, who became the landless commoners. According to the traditions, the power of the *moi* over the gentry was won by slow degrees and by hard measures. In the course of wars and re-

volts new ruling lines were created, many were deprived of their landholdings, and others received great estates (Fornander 1916). The ultimate centralization of Hawaiian government came under Kamehameha I, who, like his Tahitian contemporary Pomare I, used European help to carry out traditional political objectives.

All in all, the histories of the Stratified societies point to the sequence: Traditional–Open–Stratified.

PARALLEL SEQUENCES IN CLOSELY RELATED ISLANDS

Another interesting line of evidence on Polynesian evolution comes from a comparison of very closely related neighboring cultures. The pattern of variations shown by Polynesia as a whole apparently has been recapitulated in the histories of individual Polynesian societies and is repeated again among neighboring societies. In this connection, the situation in the Marquesas would have been extremely illuminating if detailed cultural comparisons of all the islands had been possible. Handy (1923) has pointed out that Marquesan cultures varied, the southern islands having less definite social stratification, less definite private ownership of land, and chiefs of lesser sacredness than the northern islands. The southern people, he noted, were more assertive and more independent-minded.

The fact that the variations seemingly characteristic of the area as a whole show up in neighboring islands would seem to demonstrate, first, that these differences are the result of internal developments and of migrations by which a parent culture is carried forward (and sometimes backward) in a new environment, and, second, that the conditions promoting evolution were everywhere active in Polynesia.

Among the societies that are most clearly related by tradition as well as by trait similarities are Samoa–Tonga–Niue; Maori–Tahiti–Hawaii; Easter Island–Marquesas–Mangareva. Other combinations can be considered, but these three illustrate the point well enough. The chart of Polynesian subcultures shows that closely related societies may share common traits and still have different social systems. This is because in evolution new characteristics may emerge but the bulk of changes consists of the repatterning of traditional traits.

Samoa–Tonga–Niue: Samoa and Tonga (Fiji has been omitted from what should be a triad, because of its Melanesian affiliations) are clearly sister cultures; Niuean traditions link it to immigrants and invaders from both. Samoa is the Traditional order being undermined by political ambitions; Niue is a Samoan system shattered beyond recognition by warfare; and Tonga is the evolution of the Samoan type into stratification via warfare.

Marquesas, Easter Island, and Mangareva represent variations in "openness." In Easter Island the contest for social position and power led to the exploitation of captives. In the Marquesas, where "tribal democracy" was still strong, some chiefs had begun to usurp land which they had held in cus-

tody for the tribe. In Mangareva this process had gone to the point of complete expropriation.

In the *Maori–Tahiti–Hawaii* group, Maori stands as the culmination of the Traditional order, Tahiti is a stratified phase in which the power and position of local chiefs were sustained by their inalienable land rights, while Hawaii was the Polynesian endpoint in centralized power. If we include Mangaia, which claims kinship with Tahiti, the sequence of variations is the more complete.

PARALLEL CULTURAL CHANGES

Fundamental changes in the Polynesian systems of status appear to have produced major modifications in many basic Polynesian institutions and cultural values. Some basic changes in government, land systems, and kinship have already been brought out. The following section summarizes those additional changes that seem to be most closely related to the evolution inspired by status rivalry. The correlations are not necessarily clear-cut. They refer to central tendencies and generally follow the gradations of the societies constituting a type. Thus, the more "open" of the Traditional societies are apt to show many of the characteristics of the Open societies. It was obviously impossible in this article to present detailed cultural comparisons. Finally, it should be noted that, while the ethnographic data for this section are reasonably adequate, they are not complete.

Authority

1. *Government changed from a kinship to a territorial basis.* This very significant finding has already been made by Burrows (1939). I would restate it as follows. *Traditional:* Although chiefs ruled over territories in some places, kinship was still the main source of authority. *Open:* Authority shifted from senior kinsmen to strong leaders, and, while it served the interests of a kin-unified group, it was also exercised over captives and other aliens, thus introducing a territorial principle. *Stratified:* The territorial principle was consolidated through the fuller incorporation of conquered peoples into the administrative territory. At the same time, authority and hereditary succession were strengthened to bolster the ruling dynasties.

2. *Government began to rely increasingly upon physical force. Traditional:* Chiefs rarely used force against their own people. Futuna may have been one exception. Manu'an chiefs had life-and-death powers over their subjects but rarely used them. *Open:* Force was used mainly against aliens. *Stratified:* Force, including severe and capricious punishment against one's own people, was common.

3. *Government became more highly organized. Traditional:* Government was informal and in its most elaborate forms consisted of chiefs and a council of subchiefs and perhaps priestly advisors. *Open:* Government was equally informal. *Stratified:* Mangareva, as the most "open," had, accordingly, the most

informal government. The others had developed a court and a body of specialized officials. The most elaborate was the Hawaiian court, a product not only of more complex administrative problems but of an interest in broadening the base of government by adding officials and creating new status positions.

4. *Religion became an official arm of government.* Government and religion were closely linked in all Polynesian societies and therefore, as governments developed, so did their religious sanctions. *Traditional:* Priests and chiefs worked together for community interests, and in some places chief and priest were the same person. *Open:* Rule was mainly by strong men who were less closely dependent upon the priesthood for their authority. In Mangaia, for example, priests who sought political advantage for themselves lost their religious immunities. *Stratified:* A highly organized priesthood followed very closely the political aims of the government, serving primarily the interests of the ruling dynasty—although it could also oppose them—and secondarily those of the community as a whole. Priests were political advisors and regulated the *tapu* system. The Tongan priesthood was the least organized.

5. Tapu *became a political instrument. Tapu,* as the ritual counterpart of *mana* and as a religious device for social control, was universal in Polynesia. As political power developed, *tapu* quite logically became its bulwark. *Traditional: Tapu* emphasized the sacredness of the high-born and was used to conserve crops. *Open:* Mangaia and Easter Island had begun to apply *tapu* directly in the interests of their rulers. *Stratified:* While economic *tapus* served community interests as well, they primarily sanctioned the political and economic privileges of chiefs, gentry, and priesthood.

6. Tapus *came to be enforced by physical as well as by religious sanctions.* This is a corollary to point 5. As *tapus* became political, their enforcement also became political. *Traditional: Tapu* violations were self-punishing. *Open:* Mangaia and Easter Island had penal sanctions. *Stratified:* All used penal sanctions.

7. *The attitude toward human life became more callous.* As warfare, along with conflicts for power and economic advantage, became more prominent, rulers took upon themselves god-like powers over human life. This showed itself, for example, in the attitude of the Tongan chief who cut off the left arms of his cooks simply out of caprice (Gifford 1929:127). It was best revealed, however, in the practice of human sacrifices. *Traditional:* Human sacrifices were either nonexistent or, as in Samoa and Maori, infrequent and minor. *Open:* Human sacrifices were important except in Niue, where they did not occur at all. *Stratified:* Human sacrifices were more common, and in Tahiti and Hawaii reached great proportions. Tahitian traditions say that conditions were once so good they made no sacrifices. Then things got bad, and they began to sacrifice pigs. Finally, a king told them, "We must tremble with fear," and he introduced human sacrifices (Henry 1928:127). Victims were captives, the humble, and the politically unreliable.

8. *First-fruits ceremonies became a formal taxation device.* All Polynesian chiefs had first-fruits privileges. In the *Traditional* and in the *Open* these

tributes were ceremonial, and the offerings were token symbols. In the *Stratified* the ceremonialism of first fruits continued but collections became formal, obligatory, and signs of political loyalty. The redistribution of first fruits was also universal, but in the Stratified societies this often became little more than a gesture. In Tonga, for example, the lowly were given spoiled meat considered unsuitable for the gentry (Martin 1817:132).

Property

9. *Land tenure changed from a tribal to a feudal basis.* (This point has already been discussed.)

10. *The lower orders came to bear the brunt of food shortages.* All Polynesian societies accepted some measure of responsibility for the community, but they differed in the degree to which they did so. *Traditional:* Sharing a common food supply was common. *Open:* The strong took a larger share but accepted responsibility for their followers. *Stratified:* The commoners were neglected. In Mangareva they often had no recourse against inconsiderate landlords. But, as a rule, everywhere in the Stratified societies they could leave a lord and join the estate of another.

11. *The distribution of wealth became uneven.* In all Polynesian societies the chief was the center of food redistribution. In the *Traditional* the distribution was roughly equal; in the *Open*, Mangaia and the Marquesas had uneven systems favoring the chiefs or warriors. In the *Stratified*, chiefs and gentry invariably held the largest shares. It is true that each subchief redistributed a portion of his share among his followers. By doing so, however, he bolstered his own political position.

12. *Property attitudes became more predatory.* The *Traditional* valued property for the physical comforts it provided and as symbols of rank, but chiefs were only mildly acquisitive and essentially nonpredatory. The *Open* were predatory mainly at the expense of aliens. They indulged in rivalrous ostentatious property displays. In the *Stratified*, wealth was both the price and the reward of power. Thus, land was the object of predatory attack and land boundaries were carefully marked and zealously defended.

13. *Land became alienable. Traditional:* Land was usually vested in a kinship group, and that made it inalienable except in relatively infrequent cases of conquest. *Open:* Conquest and the absorption of captives led to a more fluid tenure system. *Stratified:* Commoners had lost their traditional rights to land, while, except for Tahiti, even the gentry were no longer secure in their tenure.

Kinship

14. *Kinship bonds weakened.* All Polynesian societies maintained the theory of tribal unity through kinship and allowed commoners to trace their kinship to chiefs. Changes that occurred were more in practice than in formal structure. *Traditional:* Kinship unity was very strong, and even among the high ranks intrafamilial conflicts were occasional. *Open:* Except for Niue, which was

more like the *Traditional* in this respect, wars and the internal struggle for power realigned kin and provoked intense intrakin conflict. *Stratified:* Kin ties were strongly affected by political considerations. Discord was at its peak in Mangareva. Class lines reduced kin ties to mere formal recognition. In Mangareva a chief killed a commoner relative who had addressed him by a kin term. In Hawaii the dispersal of kin had gone farthest of all (Burrows 1939).

15. *Political and status motives in marriage became more important.* Rank was a factor in marriage in all Polynesian societies. As status and power became more important, marriage continued to be an advantageous avenue for social advancement. Marriage ritual accordingly was most elaborated among the upper ranks in the Stratified societies.

16. *Caste developed only in the Stratified societies.* Tahiti and Hawaii drew hard caste lines. Caste may be regarded as an intensification of the rank consciousness present in almost all Polynesian societies.

Position of Women

17. *Tapu restrictions on women became more severe.* All Polynesian societies regarded women as in some way defiling. In the *Traditional* and *Open* societies, *tapu* restrictions were, on the whole, minor (except for Uvea and Maori). In the *Stratified* these tended to become more severe, particularly in Hawaii where husband and wife needed separate living quarters.

18. *Women gained more political rights.* High-ranking women received ceremonial recognition in all Polynesian societies, but only in the *Stratified* (and in the Marquesas) could they become *ariki.*

19. *Primogeniture came to overrride sex in political succession.* This is a corollary to point 18.

Sexual Practices

20. *Sexual orgies became more prominent.* All Polynesian societies permitted considerable premarital sexual freedom at the same time that they required premarital chastity of high-born women. In Tahiti and Hawaii, orgiastic sexual rites increased postmarital sexual license.

Infanticide

21. Infanticide occurred sporadically in Polynesia but, where it did, it showed interesting variations. In Pukapuka infanticide was in response to population pressure, in Tahiti it was to maintain caste lines, and in Hawaii it was to avoid the nuisance of child-rearing (Malo).

Mourning

22. *Violence in mourning began to turn outward.* Almost all Polynesian mourners showed grief by beating or gashing themselves. However, only in the *Stratified* societies did mourners for a chief or a king turn their violent expressions of grief against other people as well. Tongans, for example, fought bloody

sham battles, while Hawaiian and Tahitian mourners killed or maimed anyone unfortunate enough to get in their way.

Warfare

23. *Warfare became more prominent and more serious. Traditional:* warfare was of minor importance except for Maori, Tongareva, Futuna, and Uvea. It was virtually state policy in all the *Open* and *Stratified*, where the motives were usually predatory and where the chiefs and the outstanding warriors were the main beneficiaries of military success.

24. *Combat became more cruel.* The Polynesians were not gentle warriors. However, the *Open* and *Stratified* encouraged, even by Polynesian standards, unusual acts of ferocity against enemies.

Priesthood

25. *The priesthood became more elaborately organized.* Except for Maori, priests were shamans or priest-chiefs, or else the professional priesthood was very simply organized in the *Traditional*. The *Open* (except for Niue which had no priests at all) and the *Stratified* had the most elaborate priesthoods. These reached their peak in Tahiti and in Hawaii.

26. *The priesthood became more political* (see point 4).

27. *Ceremonialism increased* (see point 26).

28. *Ritual became confined to rulers and to the upper ranks.* In part this was a logical development of the doctrine of *mana* that tended to ritualize the life of the high-born, and in part it reflected increasing stratification. In the *Stratified* societies it was leading to the secularization of the lives of the commoners.

Deities

29. *The gods became more vengeful and awesome.* The distribution of the major Polynesian deities such as *Tane*, *Lono*, *Tu*, and *Tangaroa* covered most of the islands, but the attributes of the pantheon varied significantly. In the *Traditional* the deities were apt to be either beneficent, neutral, or, at worst, mischievous. In the *Open* and *Stratified* the gods were more threatening and inspired terror by their demands for human offerings.

30. *The gods became more diversified.* This followed no absolute course. However, the *Traditional* (except for Maori) relied upon fewer deities, for the most part ancestor gods, while the *Stratified* had the greatest variety of gods and cults. The situation in the *Open* societies was mixed. Niue, Mangaia, and Easter Island were more like the Traditional, and the Marquesas approached the stratified.

Afterlife

31. *Status entered more into the conception of the afterlife.* The *Traditional* attributed no rank distinctions to the afterlife, the *Open* recognized a better afterlife for brave warriors, and the *Stratified* were the most status-conscious of all. Tongans did not even acknowledge that commoners had souls (Martin 1817,II:136).

32. *The conception of the afterlife became more unpleasant.* The *Traditional* regarded the afterlife either as more pleasant or as no less pleasant than life on earth. The *Open* and *Stratified* clearly differentiated between good and bad other-worlds and raised the prospects of a most unpleasant afterlife.

Sorcery

33. *Sorcery became more threatening.* All Polynesian societies knew sorcery. In the *Traditional*, only Maori used sorcery actively and even then mainly against other tribes. In the *Open*, sorcery was common only in Mangaia. In the *Stratified*, sorcery was common and very threatening in all.

34. *Supernatural causes of illness became more varied.* Social delinquency or religious neglect brought about supernatural punishment in all Polynesian societies. In the *Stratified*, however, the unprovoked hostility of the gods as well as human hostility in the form of sorcery added considerably to the recognized causes of illness. Chiefs were common victims of envy-inspired sorcery.

Omens

35. *Concern with omens increased.* The *Traditional* recognized some omens, the *Open* many more, and the *Stratified* were the most preoccupied with omens of impending trouble. Hawaiians, whose lives may have been the least secure, were particularly troubled about omens in building and occupying houses.

CONCLUSION

The far-reaching cultural changes correlating with the three postulated types of Polynesian society follow such a consistent course that they lend strong support to the evolutionary hypothesis that has been presented here. They show that Polynesian cultural evolution was not merely a growth in complexity—a vague concept at best—but a development of stronger political controls, more exploitative relationships, more violence, more conflict, and greater general insecurity. The material reveals how step by step the benign casual ethos of cultures such as Pukapuka and Manu'a, for example, is transformed into the fiercer and more violent outlooks of Mangarevans, Tahitians, and Hawaiians. The Hawaiians went so far as to introduce a professional outlook and a passion for betting into the traditional Polynesian devotion to sports. At the same time, we observe the growth of skills in architecture, agriculture, political administration, and in the arts of poetry and balladry. Status rivalry produced conflict and the social and psychological consequences of strife; but it also played a prominent part in promoting craft specialization, the essential precursor of technical progress.

In another positive sense the Polynesians did not accept meekly the increased pressure of arbitrary authority. They checked cruel rulers by revolts and palace *coups* and, if these measures were not appropriate, they plotted private revenges or joined with more humane overlords. It is most striking that all the Stratified societies found it necessary to teach chiefs to be " . . .

humble, kind, sympathetic, open-hearted," as Kepelino said of Hawaii (Beckwith 1932:140). Tahitian kings were warned:

> Let not the decrees of death be too frequent, for your own bones will follow the road to death. It will be like the tearing down of your own home by the warrior when the night of darkness is enveloping you, the night that hides sin. Watch your people. Arouse not that which will pain them. You may not be able to find the cure [Handy 1930:41].

In these illustrative injunctions to rulers we see the emergence into explicit form of an ethical code that runs contrary to the spirit of the moment in the Stratified societies. (Malo, who was a member of the Hawaiian court, brings this out very clearly [1951:72 ff.].) This, too, we may regard as a significant evolutionary development.

Finally, and in a more general vein, the Polynesian material shows that major cultural changes resulted not so much from the introduction of new elements but rather from a rearrangement and an intensification of traditional widespread Polynesian practices. Thus, new emergents such as "feudal" land tenure, to take but one example, had their roots in the practice of family land supervisors. Yet, once the system of land tenure had shifted to gentry-controlled holdings, the repercussions upon Polynesian society were fundamental indeed.

NOTES

[1] The present paper is part of a study in progress on status in Oceania. I am grateful to Sarah Lawrence College for a faculty fellowship that helped get the study launched. I am also indebted to Dr. Margaret Mead for helpful criticism.

[2] A proper evolutionary study of Polynesia should certainly embrace all of Oceania and Micronesia, in particular, where Yanaihara (1946) and Murdock (1948) have already called attention to evolutionary sequences. Because I regard this paper as a trial run of a particular method of analysis rather than as an attempted definitive study of Polynesian evolution, I have preferred to simplify the approach as much as possible. For this reason, I have omitted discussion of Fiji, which should ordinarily be linked with Samoa and Tonga, and I have dealt with Samoa only in terms of Manu'a. Western Samoa, more warlike, presents a somewhat different picture. I do not, however, believe that the omissions I have mentioned materially affect the main postulates presented in this paper.

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Trade, Tree Cultivation, and the Development of Private Property in Land

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COFFEE PLANTATIONS AMONG THE SIERRA POPOLUCA

IN GEORGE FOSTER'S *A Primitive Mexican Economy* (1942) there is a passage which concerns a trend toward the development of private property in land among a people, the Sierra Popoluca, who still do not recognize such ownership. The trend toward such a development is directly connected with the development of coffee plantations among the Popoluca. Coffee trees, like all other trees among the Popoluca, are privately owned. Foster tells us that "ownership comes from planting or inheriting," inheritance presumably being from someone who has himself planted. Until the development of coffee plantations, however, private ownership of trees was not leading to the development of private property in land. This was because other trees were never grown in concentrations of orchards or plantations. Ownership of the tree was recognized presumably because the individual who had planted it had a right to its fruits just as he had the right to the fruits of a harvest of maize or any other crop which he had planted, the only difference being that in the case of a tree there was not merely one harvest but a continuing series of harvests.

But with the introduction of coffee cultivation a new situation began to develop. Foster (1942:82) describes it as follows:

> Land planted in coffee is under intensive cultivation, as is a milpa; it is not a question of scattered trees here and there. Unlike the milpa, however, the land is not abandoned after several years—coffee trees last many years, and when old are replaced by new ones. Thus there is a force at play tending to perpetuate occupation of one plot of ground by a man and his children, and the logical extension is true land ownership. The first step actually has been taken. Miguel Hernandez recently bought 300 young trees . . . for 150 pesos. Payment was theoretically for the trees, at 50 centavos each. Practically it is difficult to distinguish this from the purchase of the land, since no one else can have a claim on it as long as the trees are maintained, as they are sure to be. . . .

It will be noted that the fact that coffee was traded to the outside world was the stimulus which led to the development of coffee plantations. Foster says: "Coffee is the export product most markedly dependent on the outside . . . world market. . . . Price depends largely upon the world market . . ." (1942:52). If this stimulus had not existed coffee plantations, and the accompanying tendency toward the development of private ownership in land, would presumably not have come into being—at least not as the result of a process *internal* to Popoluca society. (Foster does mention another trend toward the development of private property in land among the Popoluca [1942:81], but this too is dependent on the money economy of Mexico and the Western world

and, moreover, seems not to have any of the developmental implications of the situation under discussion here.)

THE EFFECT OF THE DEVELOPMENT OF PRIVATE PROPERTY IN LAND

The trend toward the development of private property in land had not yet, at the time of the Foster study, led to the development of true economic classes among the Popoluca of Soteapan, principally because the remaining communal lands were still able to provide food for all.

However, for another area of Mesoamerica Wagley (1941) has already demonstrated that the development of private property in land, in this case imposed from the outside by the Guatemalan state, has led to the development of incipient economic classes in the space of approximately sixty years. In the tiny community of Santiago Chimaltenango in the Guatemalan highlands, the Guatemalan state instituted private property in land in 1877 or thereabouts. Before that time a man received as much land from the town's communal lands as he could cultivate and needed to support his family. A man with more sons could cultivate more and at the same time needed more so he received more land (pp. 60–62). One of the Chimaltecos Wagley interviewed was quoted as saying, "Formerly, there was no necessity to work for others because all could get . . . land . . . " (p. 72). When communal ownership of the land was abolished, everyone in the village was given title to the land he was then occupying. Because of the inevitable unevenness of yield of different pieces of land, some were left in possession of more productive land than others. Gradually those with the least productive land were forced in bad years to sell part of their land to provide their families with enough food. And as the process progressed it became self-accelerating, for with each partial loss in land the difficulty of holding on to the remainder increased. By 1937, 12 per cent of the men of Chimaltenango owned 36 per cent of the land. By contrast, 65 per cent were dependent to a significant extent on wage labor to provide their basic needs (pp. 72–73). And the entire process took place in a community that was to a considerable extent isolated from the economy of the remainder of Guatemala—to such an extent, at least, that no outside influences can be invoked as having been the causes of the development. The establishment of nearby coffee plantations, for example, undoubtedly accelerated the development of wage labor within the village (p. 72, fn.) but was not itself a cause of the development of what might be called incipient land-owning and landless classes. The direct cause of that development was, as we have seen, the establishment of private property in land within the village by the Guatemalan state.

Wagley's study is thus a documentation of the development of incipient classes in an isolated society as the direct result of the institution of private property in land from the outside. In the light of this, the potential significance of the situation described by Foster among the Sierra Popoluca is heightened. For as we have seen, among the Popoluca private property in land is developing, *not* as the result of direct intervention of any sort by the Mexican state, but instead is clearly developing *internally* to Popoluca society as the result of

what we have called the stimulating action of trade on that society. Therefore, if the process described by Foster for the Popoluca can be shown to have widespread existence, we shall be on our way to an understanding of at least one of the sources for the development of class societies in Mesoamerica and presumably elsewhere as well.

TREE CULTIVATION IN MESOAMERICA AND ELSEWHERE

The first step in any examination of the problem of tree cultivation in Mesoamerica is the determination of the distribution of the concept of private ownership of trees there. Even a cursory glance at the ethnographic record reveals that the concept was not confined to the Popoluca. Gaspar Antonio Chi (Tozzer 1941:230) makes it clear that among the Maya of pre-Conquest Yucatan, fruit trees were privately owned even though the lands generally were held in common. Roys (1943:28) confirms this by stating that a son inherited from his father "improvements to the land" which consisted of houses and trees, both individual trees and groves of trees. Land itself was not inherited, but what man put on the land or caused to grow on it was. Cogolludo (Tozzer 1941:96) speaks of the "sale of slaves or . . . cacao plantations. . . . "

Tax (1952:60) implies some sort of a tradition of private ownership of fruit trees in parts of Mesoamerica. Beals (1945:106) implies that among the western Mixe of Oaxaca coffee trees are privately owned, but since he does not make clear what the land tenure practices are in the areas where they are grown, no conclusion can be drawn. There is a hint in Bevan (1938:42) that among the Chinantec of Oaxaca coffee trees are privately owned, but he does not discuss land tenure *per se* so again nothing can be concluded. Thus, though the evidence is admittedly fragmentary and inconclusive, what has been cited is enough to indicate that such a tradition may, in fact, have existed in Mesoamerica and that a full investigation of the matter is warranted.

Does the concept exist elsewhere? Foster (1942:82) notes its existence among "certain African and Oceanian groups." Herskovits (1952) indicates that the practice existed in East and West Africa, in India, in Melanesia, throughout Polynesia, and in the New World in the southwest and northern areas of aboriginal North America as well as in Mesoamerica. His account of the distribution of the practice merits quoting at length:

> Of significance is the point that ownership of trees, quite independent of land tenure, exists most often where the tree is of a domesticated variety that must be planted and cared for until it matures. Coconut trees in Polynesia are of this type, and so are the palm trees of West Africa.
>
> Not so well known as either of these examples are the conventions regarding the ownership of trees in East Africa. The member of the Wabena tribe, for example, regards his fruit trees as inalienable property, whether or not he retains the land he has worked when he planted them, and even if he moves to a distant village . . . [p. 377].

> In the New World trees are similarly held. Concerning tree holdings of the Popoluca of Mexico, we learn that "ownership comes from planting and inheritance"; and no instance was discovered "of claiming trees already growing in a wild state, though it is

possible this practice exists." [Among] the pre-European Ojibwa . . . an individual marked by blazes the limits of his claim . . . of maple-sugar trees . . . , which comprised as many trees as he could care for. He always knew the exact number that were his, and this was important, for unclaimed trees were free until worked, when they became private property. Trees were inherited like other goods, could be given away or loaned, and were treated in all respects like any private possession . . . [p. 378]. [Among the Navaho] if a man planted a tree it was his, but should a tree that grew of itself be needed for firewood, anyone might enter a field to chop it down without being guilty of trespass . . . [p. 362].

The Melanesian who plants trees holds them as property distinct from land, and the knowledge of who owns the trees on a given plot is "most minute and accurate." Codrington, in recounting his purchase of some land in the region, states that after the transaction was completed the owner of a fruit tree on land acquired earlier "put in his claim, which he had before omitted to make. He was accompanied by the owner of the ground on which the tree stood, who testified that the claim was good, for the claimant's grandfather had planted it." In Assam bamboos, like trees marked by a person, when small are private property, "belonging, as a rule, to the man who planted them and his heirs, irrespective of the ownership of the land on which they are planted." It is common for bamboos to be planted near the village on land belonging to someone other than the planter, and if this was not forbidden before the planting was done, the owner not only might not uproot the bamboos but was required to clear a fireline to protect them when he burned over his land. It was only away from the village, where fear of detection was slight, that such bamboos would be torn up and thrown away . . . [pp. 378–79].

[A] final instance . . . of Polynesian land tenure may be given . . . to show how the private ownership of trees, important in all this area, could be extended to include the land on which a man's trees grew. . . . In Tongareva, the coconut tree, which must be planted by man, served as a factor in accentuating "the sharp definition of individual rights to land." Since the person who planted trees on land he occupied had the right to their fruit, he was also regarded as having an exclusive right to the land on which his trees grew. So strong was this principle, indeed, that "the planting of coconuts, if allowed to go unchallenged, established the right to land even without occupation. . . . " The entire emphasis, it is true, was on the trees and the nuts they bear, yet ownership of the land was also implied . . . [p. 361].

Here, then, is a considerable body of evidence indicating a widespread distribution of the practice of private ownership of cultivated trees, the only case of the ownership of wild trees being the Ojibwa. But even the account of Herskovits is fragmentary and, tantalizing as some of his reports may be, a complete survey of the ethnographic record is indicated if the significance of the practice is properly to be evaluated. Forde (1949:233) reports still another instance of the private ownership of trees—from the North American Southwest among the Hopi, where he tells of the private ownership of peach trees. Since the concept would seem to be an outgrowth of cultivation itself, it would seem that the chances of its widespread distribution among cultivators would be great. However, such assumptions can be dangerous, and only an actual survey can settle the matter.

Returning to Mesoamerica, we can next ask whether any trees were cultivated there in pre-Conquest times comparable in economic importance to the coffee tree today. First and most important there was cacao—so important that it will be necessary to devote considerable space to it. Second, there was maguey, a plant which must be grown for eight years before it will produce pulque, the popular fermented drink of ancient and modern Mexico (James 1950:598). Since maguey as a crop must occupy the land for a number of years, it is in the same effective category as a tree. And maguey was a plant of major economic importance in central Mexico. Vaillant (1944:130) tells us that its fibers were used to make rope and twine, to weave containers, and to make clothing. Its spines were used as needles, and its leaves used as a roofing material. And its fibers seem also to have been used for the making of paper for the books of the ancient Mexicans (Kirchhoff 1952:24). As an indication of its economic importance, Sahagún (1932:233) speaks of "owners of maguey plantations." Although we will pursue no further the subject of maguey, enough has been said to indicate that it was a sufficiently important crop to warrant investigation along these lines.

CACAO IN ANCIENT MESOAMERICA

Guatemala and Chiapas

When we examine the role of cacao in Mesoamerica, two principal conclusions emerge: that cacao was widely cultivated on both the eastern and western shores, and that it was widely traded in large quantities during late pre-Conquest times. Sauer (1950:538) tells us that it was cultivated on the Pacific coast of Mesoamerica from the province of Nicoya in Costa Rica north to the Río Grande of Tepic in Nayarít province, Mexico. It had a similar latitudinal extent on the eastern coast of Mesoamerica, he tells us, but in general it was less significant in the east. As we shall see in a moment, what Kirchhoff calls "contact ethnography" indicates otherwise for the east coast (1952:267). Returning for the time being to the Pacific coast, however, we find that Shook (1951:99) reports as follows:

> Not previously emphasized is the Pacific coastal plain of Chiapas and Guatemala. Manifestations of the Pre-Classic Horizon are, I believe, greater in this region than perhaps anywhere else in Meso-America. Here, archeological sites of enormous size are situated along the banks of live streams from the foot of the volcanoes almost to the beach. Many of these were constructed entirely in Pre-Classic times. . . .

Shook continues in a speculative vein and suggests that, among other plants, cacao may have been cultivated at that time or shortly thereafter on the basis of its known later importance when virtually the same area formed the famed province of Xoconochco in Moctezuma's realm, said to produce the finest cacao in Mesoamerica. Barlow, in his study of the Moctezuma tribute lists (Matricula de Tributos), reports that Xoconochco sent two hundred loads of cacao every six months as tribute to Tenochtitlán (1949:97–98). What probably constituted a "load" of cacao we shall see later.

The Pacific Coast

Farther north in the Colima area, cacao seems also to have been an important aboriginal crop (Sauer 1948:65, 93, 104–105). For Apatzingán in western Michoacan Isabel Kelly (1947:17, 25) reports that cacao was an important crop in 1580 and concludes that it was probably aboriginal because the Spanish seem to have exercised little direct effect on the economy of the town by that date. Cacao was also grown south of Zapotitlán in Jalisco (Kelly 1949:6–7, 26). The province of Cihuatlán, which stretched from Acapulco north to the mouth of the Balsas, provided eighty loads of cacao annually as tribute to the Mexicans (Barlow 1949:14). A final point about cacao cultivation on the Mesoamerican Pacific coast: Sauer (1950:539) tells us that irrigation would unquestionably have been necessary for its cultivation in that area. He says:

> Cacao is the only crop grown between Perú and Sonora for which irrigation is certainly known to have been employed. The principal centers of cultivation lay on the Pacific slope, commonly in small valleys and on cones at the foot slopes of the mountains. Most of these old cacao areas have a limited period of summer rainfall, but the trees require wetting at intervals through most of the year. Characteristically, therefore, water was carried by small ditches through the groves. In general the stubby cacao trees were provided with a canopy of partial shade by interplanting a somewhat taller tree, usually a feathery leafed leguminous tree. . . .

There is no indication that these areas of irrigation were particularly extensive, but actually very little is known.

Honduras

Turning now to the Gulf and Caribbean coasts, we find much evidence that cacao was a crop of considerably greater importance than it was anywhere on the Pacific with the significant exception of the province of Xoconochco. Most of the evidence comes from Roys' invaluable study, *The Indian Background of Colonial Yucatan*. First of all, as far as the Maya themselves were concerned cacao was not an important crop in Yucatan except in the eastern province of Chetumal. It was, however, an important food in northern Yucatan, although something of a luxury, and was imported from Tabasco and Honduras by sea and from Chetumal by land (1943:40).

The principal centers of cacao production in the area which Roys calls Honduras were located in what are now the Caribbean coastal regions of Guatemala and in the north coastal regions of Honduras as far as the Río Ulua, which forms the southern boundary of Mesoamerica in this region. The two principal rivers along which cacao was grown in Guatemala were the Río Dulce and its tributaries and the Río Motagua. At the mouth of the Río Dulce was the important commercial center of Nito. The town was frequented by Chontal, Chol, and Yucatecan Maya traders, but Roys speculates that it may actually have been a Nahua trading post. He tells us that the importance of Nito was due to the extensive cacao-producing areas accessible by water traffic. Close relations existed with the Chol on the Sarstoon River, which forms the present

southern border of British Honduras. Considerable commerce also existed with the Chol, Kekchi, and Pokonchi peoples in the two major tributaries of the Río Dulce, the Polochic, and Cahabon. The Polochic was navigable for a distance of about one hundred miles (as far inland as La Tinta) and contained some of the best maize land in Guatemala. Cortés commented on the attractive cacao groves and fruit orchards that he observed on either side of this river (Roys 1943:114–15). The third commercial tributary of Nito was the valley of the Motagua. The country near Nito was not thickly settled, but in the Motagua basin to the southeast the Spaniards found a number of prosperous settlements with maize fields and cacao groves, and it is quite probable, says Roys, that these plantations extended for a considerable distance up the river. The Motagua was navigable for a distance of more than seventy-five miles (as far inland as Gualán). At Zacapa, a day's journey overland up the Motagua from Gualán, the cacao was less plentiful than farther down the river but was reported to be of a very fine quality. Zacapa was about twenty-five miles from the ancient city of Copán, which was situated on the Río Copán, a tributary of the Motagua. Quirigua was situated on the lower Motagua (Roys 1943:115–16).

The other major cacao-producing region in Honduras was along the Chamelecon and Ulua Rivers, with the Nahua town of Naco in the Naco Valley near the Chamelecon serving as the principal commercial center for the area (Roys 1943:116–17). Early historical accounts characterizing Naco as a Nahua site have been confirmed by archeology (Strong 1940:380).

Tabasco

Tabasco was perhaps an even more important center of cacao cultivation than Honduras. Here is Roys' account (1943:104):

> The most important product of Tabasco was cacao, for which the soil and climate are especially favorable. The trees require a hot damp atmosphere and are planted in groves shaded by other taller trees. The kernel was so highly esteemed as an article of food that it served as a medium of exchange in many parts of Middle America. It was grown everywhere in Tabasco except in Zoque towns, where the weather is said to have been too cool. The cacao groves took much attention. In addition to the work of planting, caring for the trees, and gathering the crop, the grove needed continual watching, while the fruit was maturing, to protect it from the menace of monkeys, squirrels, parrots, and other animals and birds. The crop was an abundant one. In 1579, we are told, "he who gathers least gets ten or fifteen *cargas* or more, and others as much as fifty or more." The carga, or load, was probably about 42 kilos. . . .

Note the reference to what probably constituted a "load" of cacao in 1579. There is reason to doubt that the cacao load was this heavy in preconquest times. Díaz (Maudslay 1908–16, I: 166, 211) indicates that the normal load carried by bearers at the time of the conquest was 50 pounds (two *arrobas*). Presumably the load of cacao would have been of approximately the same weight. Motolinía (1950:218) reports that the cacao load contained 24,000 cacao beans.

VERACRUZ

Moving north along the Gulf Coast we find other evidence of the cultivation of cacao in the Codex Mendoza, Stirling, and Kelly and Palerm. The Codex Mendoza (1938, I: 79–80) reports that 200 loads of cacao, in addition to a long list of other tribute items, were sent to Tenochtitlán from the province of Tochtepec. Barlow (1949:93) describes this province as including parts of northeastern Oaxaca, southern Veracruz and Tabasco, but his map shows the province as falling far short of Tabasco. Included in the province of Tochtepec was the town of Tlacotalpán. Tlacotalpán is near the Tres Zapotes site and for this reason is mentioned by Stirling, also in connection with the Moctezuma tribute lists. He says (1943:5):

> Tlacotalpán and 21 other towns of the region were required to pay tribute to Montezuma in the form of fine clothes, women's garments, weapons and insignia, jewels of gold, ornaments of amber, cacao, precious stones and feathers, fine liquid amber, and "skins" made of rubber. . . .

Immediately to the north of the province of Tochtepec was the province of Cuetlaxtlán. This was the province in which Cortés landed. The modern city of Veracruz is situated in its center. According to Barlow, two hundred loads of cacao were sent to Tenochtitlán annually from this province (1949:91, 92). In central Veracruz north of Cuetlaxtlán was the province of Quahtochco. Twenty loads of cacao were sent annually to the Mexicans as tribute from this province (pp. 89, 91). Cacao seems also to have been grown at the time of the Conquest among the Totonac of Papantla in central Veracruz (Kelly and Palerm 1952:140), but either it was not an important crop or the Mexicans were unable to exact it as tribute, for Barlow makes no mention of cacao among the tribute items which he lists as coming from the province in which Papantla was located (1949:58–60). Barlow lists still another source from which cacao was exacted as tribute by the ancient Mexicans of Tenochtitlán which is most curious for it was the city of Tlatelolco, the sister city of Tenochtitlán situated on the northern half of the island on which Tenochtitlán itself was located. He tells us that Tlatelolco provided 160 baskets of cacao ground with maize to Tenochtitlán (p. 131). Since the cacao could not have been grown in the Valley of Mexico and since Tlatelolco held no areas in the lowlands subject to it, the cacao provided as tribute to Tenochtitlán must have been brought to Tlatelolco as a result of trade with cacao producing areas.

CACAO AND LAND TENURE IN THE YUCATAN

Earlier in this paper it was mentioned that Gaspar Antonio Chi had reported fruit trees as being privately owned in ancient Yucatan. It should here be noted that he specifically included cacao trees as well as fruit trees (Tozzer 1941:230). When Roys was quoted as having reported the inheritance of trees, he likewise specifically mentioned "cacao groves" (1943:28). At still another point Roys reports that Maya rulers had cacao groves of their own

and that they used slaves which were their property in working them (p. 61). From several sources, then, we have a definite indication that cacao groves were private property in pre-Conquest Yucatan. Unfortunately, there seems to be little ethnographic evidence about this problem of tenure in other parts of Mesoamerica where cacao was grown.

TRADE

Since trade may have been an important factor in the development of cacao as a major crop the subject should bear some examination. A good introduction to the subject is Roys' description (1943:3) of the trading vessel encountered by Columbus during his fourth voyage in 1502:

> When [Columbus] stopped at Guanaja, one of the Bay Islands off the northern coast of Honduras, he came upon a large native trading craft, by which he was deeply impressed. This was "an Indian canoe, as long as a galley, and eight foot in breadth, laden with western commodities, which it is likely belong'd to the Province of Yucatan." Over the center was a canopy of mats, which sheltered the cargo and some women and children. The merchandise consisted of cacao, copper plates, hatchets and bells, flint-edged wooden swords, and a considerable quantity of colored cotton garments. . . .

The commerce of central and northern Yucatan with Tabasco and Honduras had to be carried on by water because of the lack of adequate overland routes. There seems to have been quite a considerable trade between Tabasco and Honduras, on the one hand, and the Maya of Yucatan, on the other. The main exports of the Maya were salt, cloth, honey, and slaves. Salt was apparently an export of substantial importance, for Roys tells us that the Yucatecans enjoyed a virtual monopoly of the salt business on the Atlantic seaboard. He states also that it seems probable that salt was shipped directly by water from northeastern Yucatan to Honduras. The principal exports of Honduras were cacao, obsidian, and quetzal plumage. The principal export of Tabasco seems to have been cacao, although it is likely that jade and precious stones were also exported (pp. 53–54, 56, 106–107).

Roys tells us that, in addition to the trade of Tabasco and Honduras with Yucatan, there seems to have been considerable trade directly between these two cacao-producing areas. An overland route is known to have existed, and Roys feels that there is a definite indication that there was direct sea communication between the far-distant regions. The basis of this trade is not clear (pp. 55–56).

There also seems to have been considerable trade between Tabasco and the Valley of Mexico. We have seen that Barlow referred to the province of Tochtepec as including parts of Tabasco. However, as was pointed out, Barlow's map of the Mexican realm does not indicate that the province did in fact reach Tabasco. Roys cites conflicting sources with regard to the allegiance of several of the towns in Tabasco, but makes it clear that most of Tabasco was clearly not under Mexican jurisdiction. Mexican merchants were well received in the province. Cacao, he tells us, was the principal export of the

Tabascans to the Valley of Mexico. Slaves seem to have been a major import (pp. 99, 107).

This latter point is interesting in view of the earlier mention that Yucatan cacao groves were worked by slaves. Cacao production had apparently reached such a point in Tabasco that the cacao growers grew little besides cacao and traded for at least part of their food staples with their neighbors and with peoples in the interior of Tabasco. The latter, we are told, "brought their produce down the rivers by canoe, and in return took back salt and other commodities from the coast country . . . " (Roys 1943:106). The salt which the cacao grove owners traded to the interior had been previously obtained from the Yucatan, as we have seen.

Thus we have increasing evidence that in the province of Tabasco there had developed a specialized economy based on the production and trade of cacao. There is a suggestion at least that slaves formed the basic laboring class in this economy. This is an intriguing parallel to the use of slaves in the olive plantations of the eastern Mediterranean in ancient times, but it cannot be pursued here. Nor is a deeper examination of developments in Tabasco possible at this time, although the picture which emerges from Roys's account is sufficiently provocative to warrant deeper investigation.

It is perhaps worth pointing out that the peoples of Tabasco spoke Chontal Maya, which is closely related to Yucatecan Maya but even more closely related to Cholti and Chorti. The latter two are in fact so closely related to the Chontal of Tabasco that Roys reports Thompson as suggesting that they be considered little more than dialectic variants of the same language, a language which at the time of the Spanish Conquest extended across the base of the Yucatan Peninsula to Copán in western Honduras. There were also eight Nahuatl-speaking towns on the coast and six Zoque-speaking towns inland near the base of the sierra (Roys 1943:99; Johnson 1940:108; Mason 1940:83).

It will be recalled that 50 pounds was the probable weight of a load of cacao in ancient times. Now that we have listed the total amounts of tribute in cacao received by the ancient Mexicans, it would be interesting to arrive at an estimated total weight of the cacao which they received annually in this form. The figures by provinces give a total of 900 loads annually. Using 50 pounds per load we find that the ancient Mexicans were receiving 45,000 pounds or 22 tons of cacao annually as tribute. And in spite of this their merchants still traveled to Tabasco and Honduras to trade for more!

Cacao was not merely a food in ancient Mesoamerica but also an important medium of exchange, although not the only one. Roys mentioned that cacao beans were always counted individually in order to prevent the substitution of inferior or "counterfeit" kernels. Tozzer quotes an account from Oviedo, who was speaking of Nicaragua, to the effect that elaborate efforts were made to falsify cacao kernels by removing the interior of the kernel and substituting earth for the cacao. The counterfeit could be detected, however, by placing the individual bean between the thumb and forefinger and pressing it. If the bean were soft to the touch, then it was counterfeit. Oviedo also relates that

there was a relatively fixed relationship between a number of beans and the product bought, quoting a number of things which could be bought with a specified number of beans—everything from rabbits and slaves to the services of a prostitute (Tozzer 1941:95).

CACAO IN THE ARCHEOLOGICAL RECORD

What light does the archeological record shed on the problem of the antiquity of cacao? There is no mention of its antiquity for Copán in Longyear (1951) or for the Maya in general in Morley (1946). There is no mention of it in Stone's monograph on the archeology of the north coast of Honduras (1941) or in Strong's survey of the archeology of Honduras (1948:71–120). In Drucker's recent report on the great site of La Venta, which is situated in extreme western Tabasco, there is likewise no mention of it (1952). Similarly with the reports of Drucker (1943) and Weiant (1943) on Tres Zapotes. Yet all of these were major areas of cacao production at the time of the Conquest. The question which immediately arises, of course, is how cacao production would make itself evident in the archeological record, particularly in such regions of wretchedly poor preservation. We have already seen that Shook (1951:99) has speculated on a respectable antiquity for cacao on the Pacific shores of Guatemala and Chiapas, but there is no confirming archeological evidence. In fact the only archeological support for the possible antiquity of cacao cultivation in Mesoamerica known to me is Armillas' deduction from the murals at the Tepantitla Palace in Teotihuacán, which he discovered in 1942 (Armillas 1949:91; 1951:24). On one of these murals there appear trees, one of which he believes represents a cacao tree. Tepantitla is, I believe, Teotihuacán III in age, which would push knowledge of cacao cultivation in the Valley of Mexico back about a thousand years before the Conquest. Presumably actual cultivation would have begun in the coastal regions at some undetermined time prior to Teotihuacán III.

CONCLUDING REMARKS

Archeology, then, can now throw little light on the problem of the antiquity of cacao cultivation and its role in the development of Mesoamerican civilizations. It is possible that in the future botanical or biochemical tests of the soil may make possible a definite determination on whether or not cacao was present in a given deposit at a definite time in the past. Another possible evidence of antiquity is cultural—other pictorial representations of cacao may exist similar to that discovered by Armillas. The cacao bean may be represented or an entire tree or possibly even a grove of trees. Indeed such representations may exist on material already uncovered by the archeologist and not described in these terms. If early representations are found, there may be some question as to whether the species involved was domesticated or wild, but presumably this too would be capable of ultimate determination. However, what would be most difficult, if not impossible, to determine is the most significant information: whether cacao cultivation on a large scale was stimulated by trading it to

civilizations already established in Mesoamerica and, as a result of that trade, the cacao-producing society gradually developed private property in land and as a consequence became transformed into a class society.

Evidence of the following kind would have to be found: In an area of diversified crops, with either maize or cassava as the staple crop but with cacao as a subsidiary, incidental crop, and with a society as yet undivided into classes, trading contacts would have been initiated with an area in which civilization had already developed (presumably on the basis of irrigation). Due to this trade, which gradually would have increased in intensity, there would have come into being the same tendency toward the concentration of trees into plantations which Foster observed among the Popoluca. With the progress of such a tendency would have come a tendency toward the development of *de facto* private property in land for reasons already considered. As more and more lands fell into private hands, concentrations of larger and larger plantations would have developed. And with this development would have begun the division of society into classes. The final step would have been the development of state power.

When developed in this fashion, it becomes quite clear how difficult it would be to prove whether such a process did in fact take place. Probably not until an area of this type is studied with an intensity comparable to that with which the Virú Valley of Peru was studied will it be possible to make any definite determinations in either direction, and even then poor preservation and the obstacles presented by heavy plant growth may make determination impossible. Some more arid area than that in which cacao was grown in eastern Mesoamerica may have to be substituted. Possibly the Pacific coastal regions may offer better conditions, although, with the exception of Xoconochco, cacao production seems not to have reached the level of development there which it did in the east. It may also be possible to do something with the record of maguey cultivation, for maguey is a plant which will grow only in an arid area where archeological preservation is generally of a high order. And if we range beyond the bounds of Mesoamerica, a possible testing area might exist in the Central Andes where coca plantations seem to have played an important part in cultural development. Ranging still farther, the ancient Near East raises interesting questions, for there olive trees and grape vines were cultivated for the widespread trading of olive oil and wine.

Such an analysis might shed a good deal of light on the Olmec or La Venta civilization of Tabasco and southern Veracruz in Mesoamerica, to say nothing of its possible implications for other parts of the world as well. Drucker has placed La Venta, not in the Archaic or Middle Formative where it was first believed to belong, principally because of its relationships with Tlatilco in the Valley of Mexico, but instead in the Classic or Florescent, because the figurine types and over-all ceramic patterns at La Venta are of the type found in Middle Tres Zapotes, a Classic period site (1952:131–32). This leaves unanswered the question posed by Tlatilco, but that is essentially another problem and one we cannot go into here. In any case, Drucker's dating would place La

Venta with all of its manifestations of an elaborate class society in Teotihuacán II and III times. Presumably class society had already developed in the Valley of Mexico in Teotihuacán I times, if not before—certainly we cannot assume that the monstrous Pyramid of the Sun could have been built by any other than a class society. Presumably late Monte Alban I also represents a class society. If so, then the development of La Venta is subsequent to each of these civilizations, and the question arises as to whether trade might not have been instrumental in its development. There is also a question as to whether any such development in Tabasco and possibly also in Honduras had any bearing on the development of the great Maya civilizations in the Petén. Copán and Quirigua, and particularly the latter, are in the area of Honduras (Honduras in the sixteenth-century sense) that was later known to be a major cacao-producing center. The people of Tabasco spoke a dialect of a Maya language which stretched through the Petén all across the base of the Yucatan peninsula in the sixteenth century. An important Maya site, Comalcalco, was situated in Tabasco itself. The masks on Temple E-VII-Sub at Uaxactun show clear relationships with the style of La Venta (Drucker 1952:220–21). There are also possible relationships between La Venta and coastal Chiapas and Guatemala (Drucker 1952:223), the major center of cacao production on the Pacific.

A final comment of a more general nature: Definitive determination of the role of trade in the development of class societies in Mesoamerica could well prove to have significance beyond the boundaries of that area. For example, the known connection between the development of the first class societies based on irrigation in the Near East and the secondary class societies which arose on their fringes may have been the product, at least in part, of a process such as that examined in this paper.

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On the Correlation of Phases of Culture

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IN A recent article in this journal, Phillips and Willey (1953:628) proposed an "operational basis for culture-historical integration" which makes use of "two static formal concepts *component* and *phase* and . . . two fluid historical concepts *tradition* and *horizon style*." As Phillips and Willey point out, the first two of these concepts are not likely to arouse much disagreement. Some archeologists will prefer, in particular studies, to concentrate upon artifact types or other elements of culture, but it would seem reasonable to accept *components* and *phases* (i.e., the foci of the Midwestern Taxonomic System) as the basic units of archeological research, since these correspond, as Phillips and Willey point out, to the community and society (i.e., tribal group) in ethnology.

The establishment of relationships among the various phases or groups of components is another matter. While Phillips and Willey (1953:630–31) favor the use of *traditions* and *horizon styles* for this purpose, they concede that many archeologists will prefer other concepts. The purpose of this article is neither to advocate any of the other concepts nor to propose new ones but rather to examine and evaluate the various ways of relating phases.

COMPONENT AND PHASE

It is important at the outset to gain a clear idea of what is meant by *component* and *phase*. Phillips and Willey (1953:619) have defined a component as "the manifestation of a given focus [i.e., a phase] at a specific site." It may embrace the entire site or, if there has been more than one occupation at the site, it will correspond only to a part of it—a layer of refuse, analysis unit, or whatever other division is made. It comprises all the remains encountered within this division. Hence, it is objective and has physical reality. If one wishes, one can point to a particular set of structures, group of burials, food remains, collection of artifacts, etc., coming from a designated part of a specific site, and say that these constitute the component.

By contrast, a phase is an abstraction from the reality of the components. Phillips and Willey (1953:620) quote Kidder's definition of it as "a cultural complex possessing traits sufficiently characteristic to distinguish it . . . from earlier and later manifestations of the cultural development of which it formed a part, and from other contemporaneous complexes." In other words, it is a complex of traits which recurs from component to component and which serves to distinguish its particular components from others. It comprises "arbitrary peculiarities of implements, weapons, ornaments, houses, burial rites and ritual objects [that] are assumed to be the concrete expression of the common social traditions that bound together the people" of its components (Childe 1950:2).

To use an analogy, the relationship between a phase and its components is like that between a pottery type and the sherds of that type. Just as a type, in the conception of many archeologists, comprises the attributes distinctive of a class of similar potsherds, so a phase may be said to consist of those attributes which are shared by a class of similar components. And, just as a type is defined without regard to its specific position in time and space, so a phase is basically nontemporal and nonspatial. One does not need to know its chronology in order to define it although, of course, one may give its distribution in time and space after it has been defined. It is this point which distinguishes both type and phase from the corresponding ceramic and cultural periods; chronological position is not inherent in either type or phase as it is in a period.

Finally, neither a pottery type nor a phase carries within itself the implication of specific genetic connections with other types or phases. Both type and phase are conceived of as independent entities, although it may be possible subsequent to the definition of either of them to show that it was derived, wholly or in part, from some other type or phase and thus to establish its genetic relationships.

THE CORRELATION OF PHASES

The problem, then, is how to correlate a series of phases, or recurring complexes of culture traits. There are, I would suggest, three ways of doing this, which will be called respectively descriptive, distributional, and genetic. In the first of these, the phases are related entirely *per se*, i.e., in terms of their distinctive attributes. In the second and third, they are instead correlated in terms of factors external to themselves—time and space in the case of the distributional approach, and genetic connection in the case of the third approach. Each of these approaches will be considered in turn.

Descriptive Correlation

The first approach seeks to answer the question: What phases most closely resemble each other? The answer is obtained by classifying the phases into one or more series of groups, as diagrammed in Figure 1. Phases are grouped together because they share the same attributes, and hence each group reflects descriptive similarities among its constituent phases. It may also reflect contemporaneity and genetic connection, but not necessarily so. For example, two phases grouped together on a descriptive basis may have been successive rather than contemporaneous, and their similarities may be due to adaptation to the same type of environment or to a correspondence in level of cultural development rather than to genetic connection. Additional research is necessary before one can give a proper interpretation of similarities among phases which have been descriptively correlated, and the interpretation will not always be the same. Each individual similarity has to be considered on its own merits.

The descriptive approach is thus on a low level of interpretation. But at the same time it has a high degree of validity. Its groupings are based directly

on the facts and are not subject to assumptions of chronological position or genetic relationship as are the groups to be discussed below.

The Midwestern Taxonomic Method is a good example of the descriptive approach (e.g., McKern 1939). In it, phases (*foci* in its terminology) are grouped on the basis of their resemblances into a hierarchy of aspects and larger units in the manner indicated in Figure 1. To be sure, some archeologists who have used this approach have made the mistake of assuming that the relationships thus established *necessarily* indicate a particular type of time-space or genetic relationship. However, the originators of the method warned specifically against this error; for example, McKern (1940:20) pointed out the necessity for setting up a separate time chart (although he does not

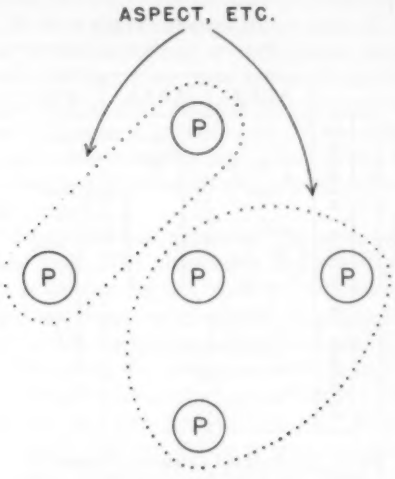

FIG. 1. Descriptive correlation of phases (P).

mention the dimension of space), and he also specified that genetic connections should be established independently. Numerous instances could be cited where this has been correctly done (e.g., Krieger 1946; Smith 1950).

The Midwestern Method has been regarded by some as a temporary expedient, designed only to meet the immediate lack of chronological information in the Middle West, with the idea that it would be replaced by the distributional approach as soon as chronology became available. It does, perhaps, have its greatest utility as a preliminary method of correlation, to be made before one has the factual basis needed to proceed with the distributional and genetic approaches. But, in addition, I would contend that either the Midwestern approach or some less systematic method of descriptive correlation—such as the kinds of statistical classification suggested by Kroeber (1940) and, in the case of ceramic attributes, by Spaulding (1953)—is a necessary pre-

requisite to proper correlation on the distributional and genetic levels. One needs to know as much as possible about the nature of one's phases before proceeding to the other levels of correlation, and the descriptive approach is a means of obtaining this knowledge, in that it brings to one's attention the principal similarities and differences between one's phases. Furthermore, descriptive correlation provides an opportunity to test the phases—to determine whether any two of them are so much alike that they should be consolidated into a single phase, for example. Any study is only as good as the units with which it deals; and hence, in so far as descriptive correlation helps one to apprehend and to validate one's phases, it should lead to improved distributional and genetic correlation.

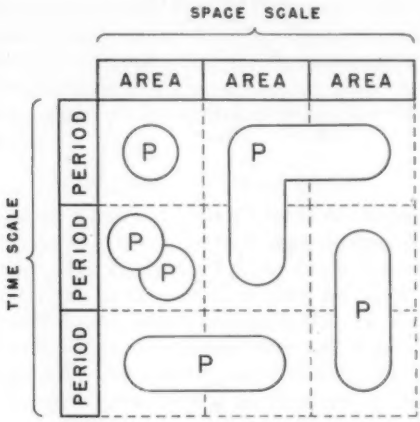

FIG. 2. Distributional correlation of phases (P).

DISTRIBUTIONAL CORRELATION

The second approach is not concerned with the nature of the phases; rather it seeks to answer the question: When and where did a given series of phases occur? The answer to this question may be obtained in several different ways but it will here be assumed, for the sake of discussion, that one does it by plotting the distribution of one's phases along space and time scales, as shown in Figure 2. Phases are placed in a certain area on the space scale and in a certain period on the time scale because the evidence indicates that they occurred there and not because of any similarities or differences among them (although the latter may be part of the evidence). The resultant arrangement cannot be considered descriptive, for the phases which are closest together temporally and spatially are sometimes quite different in content; neither can it be considered genetic, for the fact that one phase has succeeded another in an area does not necessarily mean that the former was derived from the latter. In its

pure form, therefore, the distributional approach establishes contemporaneity and contiguity, or lack thereof, and nothing else.

As is indicated in Figure 2, there may be a one-to-one relationship between a phase and its corresponding area and period; two phases may occur in a single area and period; or a single phase may extend across several areas, persist through several periods, or both. Instances of all these possibilities may be seen, for example, in the chronological charts published by Childe (1947, Tables I–III) for prehistoric Europe.

Charts like these are not easily established. Given adequate data, an archeologist should have no difficulty in locating a given phase in space, although opinion may differ as to the exact boundaries of the area in which the phase occurs. Position in time is another matter. The archeologist can occasionally obtain an absolute date (or better still, a series of them) by means of such techniques as dendrochronology or radiocarbon analysis but, lacking these, he will have to fall back upon some method of relative dating. If he is fortunate, he will be able to make use of geochronology, i.e., of a system of natural periods which, because it is independent of his phases, can be used to date them with considerable reliability (e.g., Clark 1936). More often, he has to fall back still further upon a system of culturally defined periods, based upon his phases and their content.

The so-called "direct historical approach" will serve to illustrate this last alternative (Steward 1942). The archeologist starts with the earliest known point of time, that of historic contact; seriates the phases of each local area back from this point; and then roughly equates the phases from area to area in terms of their order within the local sequences thus established, starting with the historic phases and giving due weight, as he works back in time, to differences in the duration of the phases which are indicated by depth of refuse, extent of cultural change, etc. This provides him with an arrangement of the phases according to their relative position in time and space, from which he may derive, for example, a threefold time scale of historic, protohistoric, and prehistoric periods, each with its corresponding phases or parts of phases, as the case may be (e.g., Strong 1935, Table 7).

The procedure of establishing a culturally defined time scale will not always be so regular as this. Often, it is a matter of fitting together various miscellaneous bits of chronological evidence, much as the pieces of a picture puzzle are put into their proper position. In such cases, all pertinent bits of evidence should be utilized since, for example, even when two sequences of phases are from distant areas, they may, by their consistency, serve to strengthen the validity of the over-all chronological picture. Some phases in the intervening areas may have to be left chronologically uncorrelated, because local sequences have not yet been established for their areas, but these phases can be included in the chronological chart in an indeterminate position (e.g., Kidder 1948:416, 432–33).

It is important to distinguish procedures like the above, in which phases are equated solely on the basis of their absolute or relative positions within

local sequences, from procedures in which the phases are chronologically correlated in terms of their presumed genetic connections. To say that a phase in one area is contemporaneous with phases in other areas because they share a given horizon style, for example, is on the genetic rather than the distributional level of interpretation, for it requires an assumption that the style has diffused from one phase to the others with little or no time lag. Too often an archeologist who has made this assumption will proceed to draw the conclusion that the style must have diffused from one phase to the others simply because all of the phases are contemporaneous, thereby involving himself in circular reasoning. It was apparently this danger which led Phillips and Willey (1953:624–25) to advise against broad-scale time-space correlations, and their point is well taken. It does not, however, preclude the use of nongenetic methods of broad-scale distributional correlation, such as have been discussed in the preceding paragraphs.

Even in its pure, nongenetic form, the distributional approach is on a more complex level of interpretation than descriptive correlation simply because it involves the factors of time and space. By the same token, it is less reliable. Nevertheless, it has tended to take the place of descriptive correlation as a means of ordering phases as soon as a relatively reliable form of chronology has became available. Some amateurs and art historians may still prefer the descriptive approach, since it is closer to the artifacts, in which they are most interested, but otherwise the distributional approach seems to provide a more satisfactory means of ordering or synthesizing the phases because it produces better historical perspective. It arranges the phases in the positions they actually occupied relative to each other in prehistory (cf. the "narrative approach" advocated by Brew 1946:66). Beyond this, distributional correlation is, in my opinion, a necessary prerequisite to proper genetic correlation, in that one cannot adequately demonstrate that two phases are genetically related without first showing that they have been in contact. This point will be discussed further below.

Genetic Correlation

The genetic approach seeks to answer the question: Which phases or parts thereof are derived from other phases? This is done by arranging the phases according to their lines of development. Phillips and Willey (1953:628) seem to be suggesting this when they propose the use of the concepts of *tradition* and *horizon style*, since the former expresses the development of a series of phases through time, and the latter, a corresponding linkage through space, as diagrammed in Figure 3 (*left* and *left center*).

The term "genetic" is used here in a broader sense than by many anthropologists (e.g., Brew 1946), who limit it to the phylogenetic process, whereby one phase gives rise, wholly or in part, to two or more new phases. Phylogeny, which is also diagrammed in Figure 3 (*right center*), is here considered comparable to *tradition* and *horizon style*, since all three of them imply that one phase, or part thereof, is derived from another. There are a number of other possible

forms of genetic relationship, considered in this broad sense, of which only one, the fusion of two phases to form a third, is diagrammed in Figure 3 (*right*). These various possibilities may occur singly, as is indicated in the figure, or in any kind of combination.

In order to establish a valid genetic relationship between phases, one must first demonstrate that the phases are similar and, second, that they have been in contact. In other words, one must do both descriptive and distributional correlation. Then, one may proceed to decide which phases are genetically connected and what forms the connections have taken by following out the descriptive and distributional relationships, as indicated in Figure 4.

When carrying out this procedure, it would seem advisable first to eliminate all those resemblances which do not appear to have been accompanied by contact. Next, one must decide which of the remaining resemblances are due to genetic connection rather than to some other factor such as adaptation to a

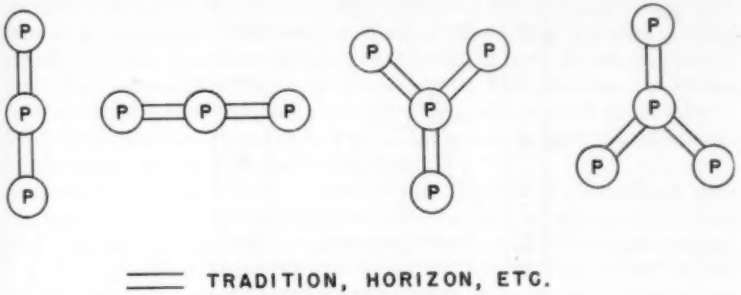

FIG. 3. Some models for genetic correlation of phases (P).

similar environment or attainment of the same level of cultural development. Only then will it be safe to choose from among the various possible forms of genetic connection, such as are diagrammed in Figure 3.

The genetic approach is on a more complex level of interpretation than either descriptive or distributional correlation and produces a more integrated picture of cultural relationships. On the other hand, it does not subsume as much of the data, since it is applicable to only one type of resemblance among phases, as illustrated above. Furthermore, it is the most difficult of the three types of correlation to carry out and the least reliable. The data are seldom adequate to indicate beyond doubt whether one phase is derived from another and, if so, in what manner—by a process of evolution, diffusion of constituent traits, phylogeny, fusion, etc. A decision among these possibilities is often a matter of opinion, as witness the current controversy whether there is a distinct Mogollon tradition, apart from the Anasazi, in the Southwest.

For these reasons, I would suggest that the genetic approach is not in itself an efficient method of ordering the phases, i.e., of making broad-scale syn-

theses of cultural relationships, although it may be useful as a check upon syntheses established by other means. It would seem prudent to make one's basic synthesis by means of descriptive or distributional correlation and then to point out the genetic relationships only in the instances where sufficient data are available and they indicate that this particular type of relationship has occurred.

CONCLUSIONS

The foregoing discussion has been concerned with three approaches to the correlation of phases: descriptive, distributional, and genetic. The attempt has been made to show that these three approaches are distinct; that they require different concepts and methods; and that they are increasingly interpretative

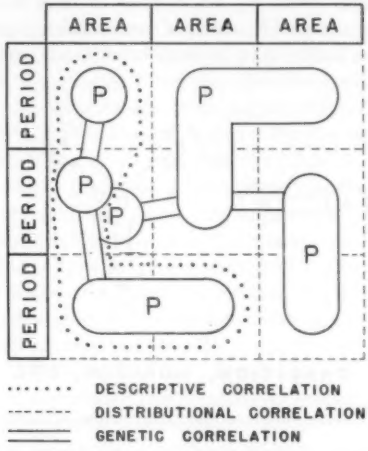

FIG. 4. Use of descriptive and distributional correlation to demonstrate genetic connection among phases (P).

in the order given, but have decreasing reliability and inclusiveness. It has been further stated that the descriptive approach is a necessary prerequisite to proper distributional correlation and that both should be done before drawing any definite genetic conclusions.

It should be emphasized that the three approaches cannot properly be carried out in terms of a single classificatory system. If one were to adopt such a system, one would have to apply it to all of the phases whereas, as we have noted, both distributional and genetic correlation have to be limited to the phases for which adequate data are available. Moreover, in order to establish a single over-all system, one must assume a single form of distribution and a single type of genetic relationship whereas, as we have seen, it is advisable in each instance to consider all the possibilities, such as those diagrammed in Figures 2 and 3 (cf. the principle of multiple working hypotheses in geology in Chamberlin 1944).

The weaknesses in many historical reconstructions are, in my opinion, the result of failure to realize this point. To take an example from ethnology, one drawback of the *Kulturkreis* approach would seem to be that it does not establish valid descriptive and distributional relationships among tribes before grouping them together genetically in *Kreise* (Kluckhohn 1936:167). In archeology, the genetic classifications produced for the Southwest by Gladwin (1934) and Colton (1939) likewise suffer from a lack of independent descriptive and distributional correlation. Both Gladwin and Colton have failed to demonstrate that their phases actually are similar, contiguous, and hence genetic; and that the relationships among them actually are phylogenetic and do not, in some instances, conform to one of the other possibilities diagrammed in Figure 3 (or to some combination of these models). Their results would be more convincing if they had done so.

When Phillips and Willey (1953:628–31) propose that the correlation of phases be made only in terms of *traditions* and *horizon styles* (and thus without recourse to the other concepts previously mentioned in their article), they would seem to be making the same mistake. I cannot believe that it is wise to apply genetic concepts like these without first correlating one's phases in terms of descriptive and distributional concepts, or to work with *traditions* and *horizon styles* except on a basis of equality with all other genetic models, as in Figure 3. To do otherwise would be to adopt a priori one form of culture change without inquiring whether the data point to another.

Culture historians should, in my opinion, beware of overrefining their concepts, methods, and results. Students of history and of natural history have long since learned that there is no short cut to historical reconstruction, and it is time for culture historians also to realize this. As Brew (1946:66) has pointed out, what we need is more, not fewer, concepts and methods, and greater use of those we already have.

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Method and Theory in American Archeology II: Historical-Developmental Interpretation

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INTRODUCTION

A RECENT paper by the present authors on method and theory in American archeology (Phillips and Willey 1953) was originally planned in two parts. The first, on space-time or historical integration, was to have been followed by a shorter part on historical-developmental theory, the two subjects being considered as related but operationally distinct branches of archeological method and theory. In the process of writing, the shorter section became the longer, and at the same time became more empirical than theoretical, winding up as an actual developmental scheme for New World archeology. Publication as part of the original paper was no longer in question and the material is therefore presented here.

Culture-historical integration, as described in our first paper, is concerned with cultural forms, the plotting of these forms in space and time, and their inferred functions in context. There is, we believe, common agreement that these are archeology's primary tasks. It is in realization of this that the archeologist defines "units of culture" for study. Our arguments in the aforesaid paper were addressed to the questions of how such units should be formed and how they may best be related one to another. This, we maintained, is foundational to further theoretical formulations in the fields of culture continuity and change. Various trial examinations and hypotheses might take off from this point. The historical-developmental analysis presented here is merely one of such possibilities.

Let us start by asking what method of organization can be used to integrate archeological data on an America-wide scale? All Americanists recognize that, whereas the archeological reconstructions of localities, regions, and areas may be treated in unified systems, we are unable to extend these systems, in any successful or impressive manner, beyond the bounds of the major "culture area." This, in itself, is a highly interesting aspect of New World culture history. The fact that the great American art styles rarely, if ever, pass beyond the limits of the area of their characterization is surely a key to a certain cultural understanding. Similarly, the relative scarcity of specific objects, "trade items," that are native to one area hearth and transmitted to another, is a clue to the areal isolationism of New World cultures. What, then, is the nature of the historical evidence that links the various American areas?

In our previous article we discussed two integrative mechanisms, the *horizon* and the *tradition*. The horizon is characterized by its relatively limited time dimension and its significant geographic spread. It is usually expressed by an art style or a very specific complex of features whose historical uniqueness

cannot be mistaken. As we have just noted, such phenomena do not frequently pass outside the major cultural-developmental areas to which they are allied. The horizon clearly has little utility in multiple area syntheses. But what of the tradition? We have described this concept as "a major large-scale space-time cultural continuity, defined with reference to persistent configurations in single technologies or total (archeological) culture, occupying a relatively long interval of time and a quantitatively variable but environmentally significant space" (Phillips and Willey 1953:628). Attention is called to the fact that, apart from the temporal factor—the significant quality of all traditions is their appreciable time persistence—this definition allows for the greatest flexibility in spatial dimensions and, above all, in content. A tradition may pertain to certain technologies (e.g., a way of chipping flint, pottery-making or farming), to idea systems (e.g., the calendrical reckoning), or to full cultural complexes (e.g., the Mississippi tradition in eastern North America). Furthermore, we maintain that the student is free to formulate traditions on all these levels to suit the purpose in hand. For example, in the Southwest the Anasazi and Mogollon have been conceived by many archeologists as separate cultural traditions; or they have in part been combined into a single "Pueblo" tradition (Reed 1950); or they have been included, along with Hohokam and Patayan, into a single "Southwestern Co-tradition" (Martin and Rinaldo 1951). It cannot be said that one of these formulations is "right" and the others "wrong"; it all depends on the angle of vision.

For purposes of hemispherical synthesis, we are therefore free to use tradition formulations of the widest variety and scale, remembering only that with increase in spatial and temporal dimensions goes a corresponding decrease in specificity of content. When the latter reaches the point of historical non-significance (a point that is difficult to determine), the practicable limits have been exceeded. It follows that what we optimistically termed "full cultural traditions"—i.e., having specificity and configuration—have little value outside the limits of the major cultural area. In fact, such areas are defined by their over-all cultural traditions, and there are clear functional interrelationships between such traditions and their natural environmental theaters. On the other hand, the technological or idea-system kind of tradition does have interareal usefulness. American maize agriculture is one of the broadest of such traditions. It has distinctive content (*Zea mays* and its hybrids) and associated technical skills related to its cultivation. Its tremendous space-time range in the New World, however, makes it unsuitable for tracing out detailed interareal relationships; nevertheless it has an important historical unity. It is an American tradition whose diffusion has overridden areal boundaries and whose presence establishes common ties. Pressure flint chipping and pottery making are traditions of even broader scope, approaching the condition of nonsignificance mentioned above. There are other technological traditions whose range is more restricted and whose appearance, therefore, may indicate a higher degree of historical intimacy. The metallurgical techniques which extend from the south Andes northward to the southwestern United States

furnish an example. These cross at least five major archeological areas but are almost certainly a historical unit. The resist-dye painting of pottery (negative painting), the use of the ceramic mold, curious and complex vessel forms such as the stirrup-mouth jar—all these are multiple-area phenomena, each presumably of single historical origin.

But can tradition phenomena, of whatever kind, be integrated into an effective historical system for the Americas as a whole? Horizon styles and horizon markers enable us to equate local and regional phases in a reasonably precise space-time scheme for a particular area. Horizontal equivalence on the space-time chart means approximate contemporaneity. It might be supposed that traditions of the widespread sort referred to above would have a similar usefulness on a scale larger than that of the area, but such is not the case. Maize agriculture, for example, did not commence at the same time, or even approximately the same time, throughout its range of distribution; nor can the Peruvian and Mexican *cire perdue* casting be established as contemporaneous. There is, in short, no device (except calendrical dating) by which cultural traditions can be temporally equated on a continental or hemispheric scale. The tradition and horizon are, we repeat, the most effective units for the presentation and interpretation of intra-areal relationships, but these are the limits of successful spatial-temporal integration. For larger syntheses another type of formulation must be resorted to, one that is free from strict limitations of space and time, yet has a general historical validity in the widest sense. The only possible kind of scheme that meets these requirements, so far as we can see, is a series of cultural stages in a historical-developmental sequence. These stages will of course be founded on common participation in important historically derived traditions, but their formulation is a procedure distinct from the methods of systematic spatial-temporal, or historical, integration as laid down in our previous paper.

Developmental classifications of this kind, pertaining to aboriginal cultures in the New World, have, for the most part, operated on an areal basis. They have followed in the wake of archeological chronologies, and they have been attempts to generalize culture change by the comparison of regional sequences from the same area. The Pecos Classification for the southwestern United States (Kidder 1931; Roberts 1935, 1937), the Ford-Willey (1941) and Griffin (1946) outlines for eastern North America, the Armillas (1948) and Caso (1953) classifications of Middle America, and the Larco Hoyle (1948), Bennett (1948), Willey (1948), and Strong (1948a) formulations for the central Andes are all essentially developmental schemes of this sort. Although the strength of these systems has derived largely from the supporting archeological chronology, this very fact has led to misunderstandings.[1] Cultural development has a time dimension, but this dimension is not necessarily uniform for all localities or regions of an area. When stage lines and horizon lines are represented as identical, and horizontal, on a chart of culture sequence and development, confusion is inevitable. It is a problem to be likened to that of attempting to extend developmental stages from one major area to another,

except that within an area the time discrepancies probably will not be as great. In Andean and Middle American archeology this problem has been rationalized by insisting upon the homogeneity of the area co-tradition and the belief that developmental change will be more or less uniform throughout the area with no regional retardations. This is an unlikely possibility and certainly not a demonstrated fact. In eastern North America diagonally rising stage lines were employed in one scheme (Ford and Willey 1941), and, apart from the circumstance that some of these lines may have been sloped the wrong way, it is likely that this conception fits better the facts of chronology and development; however, when all chronology is relative, as in this particular scheme, there are no firm landmarks in establishing relative rates of culture change from region to region. The result is that this must be done by sheer speculation.

When we shift from the areal to an interareal consideration of the New World data, we note that only two comprehensive developmental schemes[2] have been formulated with recent archeological evidence. Steward's "A Functional-Developmental Classification of American High Cultures" (1948) is a scheme for equating, on a developmental rating, the major stages of Middle American and Andean civilizations. These equations are made upon the bases of technology for the earlier stages and upon what might be called general configuration in development for the later. Although Steward's primary interest in this particular article was the areas of high cultures, his classification carries much wider applicability. He defines six stages: (1) "Preagricultural"; (2) "Basic agricultural beginnings"; (3) "Basic or inter-areal developmental or formative"; (4) "Regional developmental or formative"; (5) "Regional florescent"; (6) "Empire and conquest." In a later article, Steward (1949a) proposed a similar classification of world-wide scope. With some changes in terminology, and with a considerable expansion of later stages, this revised scheme is basically the same as the first.[3] Steward, in effect, sees the following major steps in the development of native American cultures: first, a preagricultural hunting and gathering level; second, the beginnings of agricultural experimentation; third, the establishment of agriculture and permanent village life with special ceremonial and political institutions; fourth, a climax of the agricultural pattern with resultant artistic and technical elaborations and the maximum development of what he calls "religious states"; and, fifth, the rise of urbanism, the secular state, and the formation of empires.

Krieger (1953) has propounded a somewhat similar classification for North and Middle America. This author utilizes four major stage divisions: (1) "Paleo-Indian"; (2) "food-gathering"; (3) "food-producing"; and (4) "urban life." Except for the "Paleo-Indian" stage, Krieger does not formalize names for these divisions. His concepts parallel those of Steward's in some respects. "Paleo-Indian" and Steward's "Hunting and Gathering" are, apparently, comparable. Krieger's "food-gatherers" might also fit here, or they might be considered equivalents of Steward's "Incipient Agriculture" stage. The term "food-producing" certainly carries similar connotations to Steward's "Formative," and Krieger has made it explicit that he would bracket Steward's

"Regional Florescence" and "Empire and conquest" together in the stage he has designated as "urban life."

A comparison of the two classifications brings out the somewhat different interest of the two classifiers. Steward (at least in the two papers mentioned) was focusing his attention on the areas of "high civilization" in the New World. Krieger, while not omitting data of this type, was clearly thinking in terms of the simpler cultures, particularly those of North America. The apparent conflict over "food-gatherers" as opposed to an "Incipient Agriculture" stage reflects this. Steward, with the Peruvian sequence data on the Huaca Prieta (Bird 1948a) premaize agriculturists in mind, conceived of this stage as a slow transition from hunting and gathering to agriculture. Krieger, to judge from his examples, was considering the California cultures and those of the Archaic traditions of the eastern United States as representative of the "food-gathering" stage. Actually, either concept might serve, subject to a shift in emphasis. The Huaca Prieta culture of the Peruvian coast showed a food dependence on gathering as well as on agriculture; and there is a reasonable possibility that the Archaic cultures of the eastern United States were experimenting with plant domestication.

To date, neither the Steward nor the Krieger scheme has been applied to New World data by being spelled out in any detail. It is presumed that these authors will eventually do this, and we make no claim that this discussion is a substitute for the particular interpretations of either. It is evident, however, that the ideas which they have proposed are of such wide and general interest to Americanists that various points of view are desirable. We propose, then, to formulate a comparable scheme of developmental stages of New World culture history and to subject this classification to a "trial run" by an examination of the data projected against the scheme.

In selecting criteria we are confronted with the same obvious difficulty that faced Steward and Krieger. This is the problem of establishing comparable standards of stage definition. It is clear that archeologists cannot make the kind of inferences that would enable them successfully to characterize in a functional or configurational way the earlier stages of an American historical-developmental sequence. Concepts such as "formative," "florescent," or "classic," as these have been applied to the cultures of Middle America and the Andes, have reference to over-all configurations of cultural institutions: sociopolitical and socioeconomic forms, religion, art, technology, and intellectual achievement. As stages they are, in effect, defined by "configurational" criteria. On the other hand, the stage concepts of the earlier levels—"Paleo-Indian," "Early Lithic," "Pre-agricultural," etc.—are defined by simple technological, typological, or subsistence criteria. Except within very broad limits, these definitions offer little of a functional or configurational significance. Eventually, as archeological knowledge increases and as methods of inference become more effective, it may be possible to formulate meaningful configurational as well as typological criteria for the earlier American cultures, but at present we can do this only for the later.

Since the data for the early stages of development in the New World are

largely North American, our first consideration must be to outline stages that will incorporate existing concepts in that area without undue strain. Looking at North American lithic cultures from the widest possible angle, there seem to be three major categories: (1) unspecialized and largely unformulated core and flake industries, with percussion the dominant, if not the only, technique employed; (2) the same *plus* more advanced techniques of indirect percussion and pressure flaking; and (3) both the foregoing *plus* the techniques of grinding and polishing. The first two have been generally lumped together under headings such as "Early Lithic," "Early Man," "Paleo-Indian," etc., the possibility that they represent distinct technological stages being largely ignored because there is as yet no satisfactory evidence of a time differential. The third is the familiar "Archaic."

It would be logical to set up a stage for each of these three categories of lithic materials. In respect to the first category, the unspecialized percussion industries, we have found it impracticable to do so because there is not at the present time satisfactory evidence of temporal separateness or priority for this pattern of crude lithic technology. This problem of an American "lower paleolithic" will be dealt with in a later section. For the present it is sufficient to note that we have lumped it, along with the second category, in the first stage of our classification, for which the designation *Early Lithic* is retained. As used here, therefore, the term applies to a wide range of cultural traditions, from those that seem to reflect excessively primitive economic adjustments, with the main reliance upon wild vegetable foods, to cultures in which hunting is thought to have played the dominant role, with special emphasis on large game animals, including extinct Pleistocene forms.

For the third category of lithic cultures, that which includes ground and polished stone forms, we have retained the term *Archaic*, in the belief that this word has finally come to rest in American archeology. It is often stated that Archaic represents a further hunting-gathering stage in which the emphasis has shifted to smaller game animals, and it seems also to have been marked by a sharp increase in reliance on mollusks and fish; however, the general applicability of these inferences remains to be tested.

So much for the two lowest stages in our sequence, which depend almost entirely on typological and technological criteria, chiefly stone. The third stage, the Preformative, is the stage of transition from hunting-gathering to food producing in these areas where such a transition took place. It is comparable to Steward's stage of "Incipient Agriculture." Methodologically, it is also the beginning of the transition from the use of typological to configurational criteria. The name *Preformative* seems appropriate because it implies quite correctly that the stage cannot be understood without reference to the succeeding *Formative*. It may be defined briefly as the stage in which many, but not necessarily all, of the Formative elements are present: agriculture, sedentary or at least seasonal continuity of settlement, stable relationships to locality expressed in ceremonial or mortuary undertakings, technical elaboration in stone and other materials, textiles, and ceramics. These elements

have been assembled, in whole or in part, but have not yet become functionally integrated into the distinctive New World pattern that we recognize as *Formative.*

This brings us to the fourth stage. The establishment of an agricultural, settled village type of life in nuclear America has been variously referred as to "Archaic," "Developmental," "Formative," "Pre-Classic" and "Food-producing." We think that *Formative* is particularly apt, as it implies the "formation" of the American village-agricultural pattern, and because this pattern is "formational" to later and more advanced developments. The fact that the term has been widely used in area classifications in Peru and Middle America is also in its favor.

In designating the next stage we again follow the various Middle American and Peruvian schemes, and also go along with Steward, in recognizing a Classic or Florescent development as the penultimate stage in those areas. Both terms have been used in the literature, and there is little to choose between them for they seem to mean about the same thing. As one of the present authors has already committed himself to *Classic* (Willey 1948, 1950), we will adhere to this label.

The sixth and final stage, likewise represented only in Middle America and Peru, is the *Postclassic.* This noncommittal name is again a compromise. Terms such as "Empire and Conquest," "Militaristic," "Expansionist," "Fusion," and "City-Builder" have been used for this stage or subdivisions within it. Militarism and expansionism characterize certain phases and regions at this stage, both in Mexico and Peru, but these are not the only important features or forces. Urbanism or city-building is also a characteristic of some, but apparently not all, regions. To avoid identifying this stage with any single cultural characteristic we have adopted the term *Postclassic,* which has a developmental-sequence connotation only.

This gives us a present total of six historical-developmental stages, as follows: (1) Early Lithic, (2) Archaic, (3) Preformative, (4) Formative, (5) Classic, and (6) Postclassic. The remainder of this paper will be devoted to more comprehensive definitions of these stages and to their identification, examination, and evaluation in terms of specific archeological phases and traditions, according to our definitions of those terms set forth in the previous paper.

Before embarking upon our exploration of American aboriginal culture history within a synthetic framework of historical-developmental stages, we wish to set down our theoretical attitudes and position. To begin with, it is obvious that the processes of culture change through time and space are not analogous to those of biological evolution. Because of the unique qualities of convergence and assimilation (diffusion) the archeologist will never be in the position of the paleontologist who, from a single bone, can reconstruct the total organism. This does not deny that the cultures of human societies are integrated, functioning wholes rather than random assemblages of elements, but it does negate the theory that such cultures or institutions within cultures

are necessarily transmitted as integrated wholes. The area phylogenetic systems of culture classification that we discussed in our previous article (Phillips and Willey 1953) suffer from this fundamental defect. Nor can cultural causality be explained by set deterministic relationships within the province of culture alone. Man's biological and subcultural psychological and emotional reactions cannot be omitted from the equation nor can the reality of the societies or social groups in which he exists be denied its formative role. Increases or decreases in population size and groupings and the natural environment, through what Steward (1953:320) has termed "cultural-ecological adaptations," are also significant factors exerting strong influences on cultural change and direction of change.

In a recent article Bock (1952:494) argued that the processes of culture change and evolution will not be discovered outside of history and that, "There is no reliable procedure for arriving at universals except in the presence of particulars." We are in agreement with this. As applied to a study and survey of the aboriginal New World, this means that culture development cannot be considered profitably outside a framework of culture history. Consequently, the trial formulation which we are about to test, although developmental in its implications, does not eschew history but is an attempt to provide for its own examination within a historical frame of reference. Such a historical frame of reference takes cognizance not only of sequence but of the complex interrelationships among cultural phases and traditions (diffusion). It is in this sense that we use the term "historical-developmental."

It is clearly impossible in the space available to survey the entire field of American archeology on each historical-developmental stage. The most we can do is sample liberally from all areas and periods in the hope that no significant block of data is overlooked altogether. In doing so we are only too conscious of the freewheeling nature of the enterprise. Many of our facts may be badly selected, out-of-date, or misinterpreted. It is not to be expected that the specialist in any area will be satisfied. We may plead in extenuation, however, that the purpose of the inquiry is not to effect a tight classification of New World cultures but merely to give enough examples on each level to objectify our definitions and, more importantly, to uncover the snags that are inherent in this kind of approach.

EARLY LITHIC STAGE

The Early Lithic is defined here as the stage of rough and chipped stone technology prior to the practice of grinding and polishing. "Rough" refers to artifacts shaped by use rather than design; "chipped" covers the full range from crude percussion to controlled pressure flaking. Work in bone and horn is assumed to have been important, but the evidence has largely disappeared. Settlement and habitation patterns are such as to leave few traces in the ground. There is evidence that the pit-house may have been early enough in Asia to have come into the New World on an Early Lithic level but none to show that it actually did so (Daifuku 1952). Correlated with the absence of

"permanent" houses is the scarcity of deep refuse deposits. The general pattern of life appears to have been not only nonsedentary but even fairly migratory. Evidence of long-continued seasonal or intermittent occupation of the same camp sites generally appears first in the Archaic.

Socioeconomic inferences are meager, not to say hazardous. A small-scale kinship-based type of organization supported by a hunting, catching, and gathering economy is postulated, but within this simple generalization great variability and a high degree of specialization in response to environmental requirements must be allowed for. The data do not support the view that because Early Lithic cultures are relatively uncomplicated they are also uniform. Let us not forget that the major portions of both American continents were "pioneered" on this level.

The best-known areas for the finding of Early Lithic remains are the central and southern North American High Plains and adjacent eastern foothills of the Rocky Mountains, home of a series of early hunting cultures sometimes referred to collectively as Folsom-Yuma. Available data consist of a few thin habitation sites, a larger number of game "kills" and butchering stations, and innumerable surface finds in "blowouts," where they are associated with materials of all ages, and in isolated spots without associations of any kind. Analysis of these data has so far yielded little more than a few lithic complexes and a projectile point typology (Krieger 1947; Wormington 1949), but there has accumulated a consistent body of geological and paleontological evidence, backed by radiocarbon dating, that places them in the closing episodes of the Pleistocene and early post-Pleistocene periods. The best documented "phases" are, in rough chronological order: Clovis (Howard and others 1935; Sellards 1952), Folsom (Cook 1927; Figgins 1927; Roberts 1935, 1936b), San Jon (Roberts 1942), Plainview (Sellards, Evans and Meade 1947), Long (Hughes 1949), and Eden-Scottsbluff (Renaud 1931; Barbour and Schultz 1932; Moss and others 1951). This ordering is based on the succession of extinct faunal associations and stratigraphy in the Clovis and San Jon sites in eastern New Mexico, and in the Finley site (Eden-Scottsbluff) in southwestern Wyoming, and is partly confirmed by radiocarbon dates ranging from about 8000 B.C. for Folsom to 4000 B.C. for Eden-Scottsbluff.[4] Taking into account the stratigraphic priority of Clovis over Folsom at the Blackwater No. 1 site in the Clovis locality (Sellards 1952:29), an over-all estimate for the Folsom-Yuma tradition would be about 9000 to 4000 B.C.

The older dichotomy between fluted and parallel-flaked "cultures" implied in the terms "Folsom" and "Yuma" has given way before an increasing accumulation of intermediate forms, which seem to have been intermediate in time as well. A general continuity is also reflected in the similarity of associated implement types, wherever these are known. In short, earlier taxonomic simplicities have, as usual, proved inadequate, and we have now a situation of great complexity that has not yet been subjected to large-scale integration.

Spatial dimensions of the Folsom-Yuma continuum can be established only within the broadest limits. Some of the point types, particularly the fluted

forms, generally closer to Clovis than Folsom, have a near-continental distribution. It is unsafe to specify any regions in North America where they are not found, but they appear to be rare on the Pacific Coast and in the intermontane plateau and desert areas. Only doubtful specimens have been reported from Mexico and further south. They also tend to diminish northward, a situation unfavorable to any theory of specific Asiatic origin. Perhaps the greatest number of finds have been in the Mississippi Valley and the East but, until recently, without associated complexes. Now, sites are being reported, and we can begin to speak of phases. In the Enterline phase of the Middle Atlantic states (McCary 1951; Witthoft 1952) we have a complete stone assemblage, with some very interesting characteristics hitherto unreported from any western fluted point site. A site recently reported near Ipswich, Massachusetts (Byers 1954), shows close relationships to both Enterline and Folsom. An important area for surface finds, the lower Ohio Valley and Tennessee-Cumberland regions, produced the first eastern fluted point site (Webb 1951), and it seems likely that the well-known "Ohio" or "Cumberland" fluted point will be the marker type for another phase which may also include the recently discovered Quad site in northern Alabama (Soday 1954). Unfortunately, eastern fluted points have not been found with extinct faunal associations, nor are any other dating criteria available. It is commonly held that they represent a belated extension of early western hunting societies forced out by the advent of arid conditions in the Altithermal period, but this is an unproved hypothesis. Point types related to the later parallel-flaked "Yuma" phases of the tradition have the same wide, but perhaps somewhat more northerly, distribution, which possibly reflects a northward drift of the early hunters and their quarry with the slow retreat of the late Wisconsin ice. Thus the High Plains is *fons et origo* for the Folsom-Yuma tradition, an explanation that will almost certainly prove to be too simple.

The Southwest, Great Basin, and California Desert areas, though by no means uniform, have certain environmental characteristics that appear to have been culturally decisive in the Early Lithic and Archaic stages: basin and range topography, aridity, restricted vegetation and consequent scarcity of large game animals. Subject to the qualification as to how far back into the past these conditions can be projected, this vast area was an environmental theater for a way of life markedly different from that of the Folsom-Yuma and later hunting traditions of the Great Plains, one in which the collecting of wild vegetable foods, particularly hard-shelled seeds, is thought to have played a more important part than hunting. The archeology is not quite that simple, however, for the remains of early hunting cultures are also present, as in the Sandia phase (Hibben 1941), in the lowest level at Ventana Cave in south central Arizona (Haury and others 1950), and in the Playa-San Dieguito (Rogers 1939) and Mohave (Campbell and others 1937; Brainerd 1953) phases of the lower Colorado and California desert regions, not to mention the finds more specifically related to Folsom-Yuma, of which the recent dis-

covery of Clovis points with the Naco mammoth (Haury 1953a) is the outstanding example. Furthermore, even in the Cochise tradition that displays food-grinding utensils so prominently, the earliest phase, Sulphur Spring, has abundant evidence of hunting in the form of animal bones, including extinct Pleistocene forms, on the sites (Sayles and Antevs 1941). Nevertheless, many students have yielded to the lure of a simple typological and historical dichotomy: an early hunting stage followed by a gathering stage, the latter forced upon reluctant societies by the disappearance of game in the Altithermal period (Krieger 1953). There is no question that the so-called gathering cultures held out longest in the Greater Southwestern area—indeed they formed the basis for the later food-producing cultures of the Preformative—but the time of their earliest appearance is still uncertain. The deep cave sites of the central and northern Great Basin, for example, reveal a general continuity with little change throughout, a "basketmaking-wandering-collecting" tradition for which the name "Bonneville" has been suggested (Jennings 1953), and the earliest phases of this tradition have yielded radiocarbon dates of the same order of magnitude as Folsom-Yuma in the Plains.[5] Unless these dates are incorrect, we have to conclude that both hunting and gathering traditions were already established in late Pleistocene times, and it is still an open question which came first. Undoubtedly the antithesis between them has been overemphasized. It is impossible to conceive of an early nonagricultural society that would not make the best use of whatever game and vegetable resources were available within the limits of its technology; consequently, any distinctions reflected in archeology would simply be a function of the ecological balance in a particular time and place. For purposes of large-scale generalization, therefore, concepts of "hunting" and "gathering" cultures are useful only as convenient rubrics for descriptive purposes. Both, it seems, have to be accounted for in our definition of the Early Lithic stage.

The grinding utensils of these collecting cultures have tended to absorb the most attention, but the crude percussion-flaked plano-convex implements that go with them are more interesting from the developmental point of view. They constitute a general category of great importance in Southwest, Great Basin, and California archeology, for which there does not seem to be any standard nomenclature. Rude and unspecialized, seemingly made for immediate on-the-spot use, they defy classification, appearing in the literature as axes, cleavers, pulpers, scrapers, planes, scraper planes, etc., a ***reductio ad absurdum*** of functional terminology. Many of the industries regarded as early in the Southwest and Great Basin are characterized by tools of this type, and most of those of California. An attempt to organize such a vast amount of material into a common tradition would be unwarranted. Undoubtedly there are variations of major significance and many separate traditions are involved. The point of interest here is that they all show an essentially "lower paleolithic" core and flake technology with direct percussion the dominant, if not the only, technique of manufacture. The question is whether we have here a basis for the postulation of a technological stage anterior, in a developmental

sense, to Early Lithic. Before this could be anything more than a theoretical stage, certain questions would have to be considered. Have complexes of this nature been found unmixed in specific sites under controlled conditions? Do they constitute true assemblages? Can the possibility be ruled out that they are merely partial inventories due to special conditions?

A list of such finds would be too long for inclusion here. A few typical examples are: Sulphur Spring Cochise (Sayles and Antevs 1941); the Albuquerque, Comanche Springs, and Rio Puerco finds in central New Mexico (Hibben 1941); The Tolchaco complex of the Little Colorado river in Arizona (Bartlett 1942, 1943); the Malpais or Playa-San Dieguito I culture of the lower Colorado River basin (Rogers 1939; Haury and others 1950); the La Jolla culture of the San Diego region (Carter 1949, 1950); the Topanga culture (lowest level) in the Los Angeles region (Treganza and Malamud 1950); and the Farmington complex in the Sierra Nevada foothills of central California (Treganza 1952). Some of these complexes seem to fulfil the requirements outlined above and may be tentatively regarded as true assemblages. The question, then, is whether they exhibit a "lower paleolithic" stage of technology or merely a degenerative regression due to local conditions? If the former, temporal priority, somewhere in the range of distribution, would be a reasonable hypothesis. It must be admitted, however, that the general impression is not one of great antiquity, notwithstanding the claims that have been made (Carter 1951, 1952). It could be a case of extreme marginal survival. The alternative theory of technological regression poses a very interesting problem in cultural evolution, in that it runs counter to the premise that development from simple to complex is an irreversible process. On the whole, we feel that general probabilities favor the first alternative, and that Linton's hypothesis that the industries in question represent a belated survival of a very early lithic tradition distantly related to the lower paleolithic cultures of Southeast Asia (Linton 1949) is still exciting and possibly correct. However, we do not have the temerity to set this up as a separate stage even in a trial scheme like the present.

The relative infrequency of remains attributable to an Early Lithic stage on the Pacific Coast of North America appears to confirm the widely held assumption that early migration southward from Alaska was mainly in the intermontane and high plains areas on either side of the Rocky Mountains. The La Jolla and Topanga cultures of the San Diego and Los Angeles regions, respectively, have already been mentioned. The lowest level of the sequence at Malaga Cove, also in the Los Angeles region (Walker 1937, 1951), has been compared to Rogers' Dieguito and is thought to be early, but includes a microlithic "flake drill" that is remarkably similar to the characteristic perforators of the Poverty Point culture of the lower Mississippi on a Preformative level (Ford, Phillips and Haag 1955). The enigmatic Oak Grove culture of the Santa Barbara region (Rogers 1929; Orr 1952a) is also difficult to fit into our definition of Early Lithic. The four last-named cultures have been recently gathered together, along with material from the Little Sycamore site

in Ventura County, under the designation "Early Milling Stone Cultures of Southern California," and assigned a guess-date of 3000–2500 B.C. (Wallace 1954), which, if correct, would add further difficulty to regarding this as an Early Lithic tradition.

Further north, in central California, well-developed sequences in the San Francisco Bay and Sacramento Delta regions have no Early Lithic phases; the lowest levels are clearly Archaic in terms of the definitions used here. The meager Buena Vista (lower level) complex (Wedel 1941) and the unpublished Tranquility site (Hewes 1946) are worth mentioning as possibilities, only because Early Lithic remains are so rare in this part of California. The Farmington complex has already been mentioned, and recent excavations in nearby Calveras County caves (Orr 1952b; Wallace 1952; Wallace and Lathrap 1952) suggest that the famous hoax may have too hastily settled the question of early man in this locality. North of San Francisco Bay, at the Borax Lake site (Harrington 1948a), there seems to have been an early component that includes fluted points and Lindenmeier-like scrapers and gravers, but this was not clearly differentiated stratigraphically from a typical Central California Middle Horizon context.

From here on up the coast our sources of information dry up. In northern California, the Northwest Coast, and Plateau areas, finds attributable to the Early Lithic are very scarce, partly a function of ignorance no doubt, partly because we are getting further away from the bases of our criteria and are unable to recognize Early Lithic materials when we see them. A few possibly early sites have been recently reported but in no case fully published. For example, the Lind Coulee site in eastern Washington may have an Early Lithic phase ("Notes & News," *American Antiquity* 16 [1951]:290; 17 [1952]: 281; 18 [1953]:189). A site recently reported at Cold Spring, Oregon (*ibid.* 17:281) is in the same suspended category. In British Columbia, the recently elaborated lower Fraser River sequence (Borden 1951) has no Early Lithic phase, but inland on the Nechako River a series of sites shows a stone complex, predominantly percussion flaked, that may turn out to have been early (briefly reported in *Anthropology in British Columbia*, No. 2 [1951]:7).

Obviously, North Pacific archeology is not yet well enough known to permit generalization; nevertheless it may be suggested that this is a natural area quite unsuited to a migratory hunting and collecting way of life. On the other hand, it could—and did—become a remarkably favorable environment for people who knew something about boats and had the necessary tools to build them. Though dissimilar in many respects, the same observation holds for much of the Atlantic coastal area, in which Early Lithic remains are equally scarce.

This brings us to the Arctic. We can scarcely expect to fit Arctic data into our scheme of classification without extreme difficulty. As one of the outstanding authorities has recently said, "The pattern and development of Eskimo culture are the result of a combination of geographical, ecological and cultural factors that are truly unique" (Collins 1953:201). This would apply

equally to any cultural "pattern" in the Arctic, since the unique geographical and ecological conditions can be projected indefinitely into the past, and with them the special cultural adjustments required to make life endurable in this forbidding environment. Apart from marginal phases of Indian cultures whose centers lie further south, it remains to be demonstrated that any major cultural tradition other than Eskimo has occupied the area. The only context that might represent such a pre-Eskimo tradition is the well-known Denbigh "flint complex" found on the north Bering Sea coast of Alaska (Giddings 1951; Collins 1951). Denbigh shows connections, of a tenuous nature, with Folsom-Yuma but seems to have closer affiliations with eastern Siberian cultures that are variously regarded as mesolithic or early neolithic. The latter are assumed to be responsible for the burin and microcore and blade ("polyhedral core and lamellar flake") techniques that have contributed so much to Denbigh's renown. Apart from these specialized techniques, which may be mesolithic in the Old World but do not seem to be particularly early in the New, Denbigh can be tentatively classified as Early Lithic. The single fluted point is hardly worth mentioning, the Plainview and Eden-Scottsbluff-like forms are probably significant, but the radiocarbon dating is above the upper limits of the Folsom-Yuma range.[6] This lends considerable support to Collins' belief that further information about the nonlithic aspects will bring out the essential Eskimo character of the culture.

No other Early Lithic complex has yet been reported in the Arctic area. The few scattered fluted points that have turned up have been without associations and reliable dating criteria. The general impression is that they are late and marginal to the main centers of the Folsom-Yuma tradition further south. More pervasive is the showing of the microcore and blade tradition mentioned above, and there has been some tendency to regard this as a "Paleo-Indian," or, as we would say, Early Lithic, development. However, in most cases in the Arctic where we have it in context, as in the Alaska Campus phase (Nelson 1937; Rainey 1939; Solecki 1951) and Dorset culture of the eastern Arctic (Jenness 1930; Rowley 1940; Harp n.d.), it seems to be on an Eskimo, hence from our point of view Archaic, level. It has been found in the Mackenzie Valley associated with stemmed and notched points comparable with Eastern Archaic forms (MacNeish 1954a). A good deal further south, in the mid-continental area, the technique appears in cultures on a Preformative level, e.g., Hopewell in the Ohio Valley and Poverty Point in the lower Mississippi (Ford, Phillips and Haag 1955). It seems quite erroneous, therefore, to regard the presence of this microcore and blade tradition, whatever its supposed mesolithic connections, as evidence of an early date or an Early Lithic status.

Getting back to the Early Lithic, the same unprofitable generalizations can be extended to cover interior Alaska and the entire Canadian Subarctic. The Yukon and Mackenzie valleys, which are crucial as the most likely diffusion routes in the Early Lithic stage, are regions of extreme difficulty for archeology. A few finds have been reported (Johnson 1946; MacNeish 1951,

1953), but, in so far as they can be related to Early Lithic traditions elsewhere, they seem to be relatively late in the scheme of things. We have to conclude that remains pertaining to the earliest immigrants have either not been found or, if found, not yet recognized. The origins of Early Lithic cultures in North America are still completely obscure.

We would hardly expect clarification of the problem of origins in Middle and South America. Any theory of population spread into these areas must assume north to south movements of cultures in an Early Lithic stage of development. We may therefore deal with the scanty Middle and South American finds in those areas in a summary fashion.

A burst of archeological and paleontological activity, following De Terra's Tepexpan finds (De Terra 1949), has amply confirmed the presence of man and extinct fauna, including mammoth, in upper Pleistocene formations in the Valley of Mexico, but the associated artifact assemblages remain incomplete and ambiguous. De Terra's San Juan "industry" (ten artifacts from three sites) has received some slight reinforcement from finds associated with the Iztapan mammoth (Aveleyra and Maldonado-Koerdell 1953), among them a single Scottsbluff-like point. This tenuous connection might suggest that San Juan is chronologically related to the later phases of the Folsom-Yuma tradition. It may also give slight additional meaning to the fact that good fluted points have so far not been found south of Durango. The poor example reported from Costa Rica is of doubtful significance (Swanger and Mayer-Oakes 1952). The only other evidence of possible Early Lithic cultures is the Canyon del Diablo sequence in southern Tamaulipas (MacNeish 1950), of which the first two phases, Diablo and Lerma, are said to represent hunting cultures, but the material has not yet been thoroughly described and comparisons with Early Lithic material north of the border cannot be made.

From Mexico on south through the Maya area and lower Central America reference could be made to scattered and isolated evidences of man's presence in late Pleistocene times, but it does not add up to anything of cultural significance and may be omitted here.

Identification of Early Lithic remains in South America is beset with difficulties. Technological similarities between North American lithic traditions and those presumed to be early in South America are, for the most part, of a general kind. An even greater source of trouble is the limited geological and paleontological criteria for temporal equation with North American sequences. Nevertheless there are a number of finds in South America that can be considered as relevant to the problem.

One of the best substantiated claims for a South American Early Lithic is to be found in the Magellanic sequence (Bird 1938, 1946). The lowest level, period 1, in the Palli Aike and Fell's caves on the north side of the Straits, contained, along with remains of horse and sloth, pressure flaked points of ovate triangular shape with broad tapering stems, a single unstemmed Plainview-like point (Wormington 1953), crude chopping tools, bone flakers and awls, and some disk-shaped pieces of lava. Bird's original modest estimate

of 1000 to 3400 B.C. has been considerably extended by a radiocarbon date of 6688±450 B.C. (Sample No. C-485, Libby 1952a).

Other researches into the problem of early man in the southern part of the South American continent may be compared and contrasted with the Palli Aike and Fell's Cave period 1 discoveries. In some instances there are indications of even earlier materials. Vignati's (1927) stratigraphy on the Argentine coast of Tierra del Fuego reveals a three-period sequence in which the two earlier periods have only retouched cores and flakes or bifacial percussion flaked blades and choppers. In the third level he recovered a point similar to those of Bird's period 1, suggesting that the two antecedent periods may be earlier than the entire Palli Aike-Fell sequence. It is to be noted, however, that crude bolas stones are found throughout the Vignati sequence. Along the Atlantic coast of Patagonia, Menghín (1952b) has correlated site occupations with a series of rising coast lines and has defined an earlier Oliviense complex, characterized by retouched flakes and scrapers, and a later Solanense complex, featuring single-notched base projectile points. Geological dating here suggests that both of these complexes antedate Bird's period 1.

Menghín (1952a, 1952b) has also described Patagonian cave stratigraphies in which the lowest levels (Toldense complex) appear to correlate with Palli Aike-Fell period 1 on the basis of lanceolate, Plainview-like points, but in which crude bolas stones are also present. The upper levels in these same caves (Casapadrense complex) resemble Bird's period 2 at Palli Aike in the absence of stone points and the presence of numerous bone implements along with crudely retouched scrapers and blades. This later period is dated, in relation to volcanic activity in this region, as subsequent to 6000 B.C. It appears to relate to a somewhat similar complex (Tandilense) in Buenos Aires Province (Menghín and Bormida 1950).

Probably later than the above-mentioned complexes but, nevertheless, within the range of our Early Lithic definitions, is the Ayampitin phase of the Córdoba and San Luis hills in northwestern Argentina. Gonzalez (1952) has obtained a stratigraphy at the Gruta de Intihuasi in which the Ayampitín assemblage, with its almond-shaped points and crude grinding utensils, underlies the later, Archaic-like, Ongamira phase.

We cannot leave Argentina without commenting on Ameghino's (1911) claims for the antiquity of man in Buenos Aires Province. The geological basis for these claims of what seems to be excessive age is still in dispute, and the characteristic artifacts, battered and splintered pebbles suggestive of crude hand-ax or chopper forms, have uncertain associations. In some of Ameghino's sites they seem to have been found with pressure flaked points, grinding stones, and even pottery. Bird has reported similar implements from northern Chile in contexts that are relatively late (Bird 1943).

In Brazil the lithic tradition associated with the long-headed Lagoa Santa skeletal type (Walter 1948; Evans 1950) and also found in some of the sambaquis of the southern coast (Serrano 1946) has been considered as relevant to the problem of early man in South America, but we have relegated it to the Archaic for reasons given further along in this paper.

From the great corpus of archeological data in Peru, there seem to be few finds that could conceivably be Early Lithic: (1) the long tapered-stemmed points and other implements from the Chicama Valley, which seem to have no connection with later preceramic and ceramic phases in the region (Larco Hoyle 1948:11–12, plate 1); (2) the seemingly unrelated complex featuring stemless and diamond-shaped points found in the nonceramic refuse of certain inland rock shelters near Huancayo (Tschopik 1946); and the chipped stone complex which Strong (1954) has reported from the south coast. From the northern Andes there is even less. Discoveries have been claimed as evidences of man in an early period, such as the Punín skull (Sullivan and Hellman 1925) or the Alangasí mastodon (Uhle 1930), recently demolished by Gross (1951:104), but no artifact complexes that can be measured against our criteria for the Early Lithic stage.

Looking over this hastily assembled South American data, two things stand out: first, the tendency for Early Lithic finds to be concentrated in the southern parts of the continent, which supports the theory that early migrants passed along down the Andes and Pacific coast and fanned out into the Argentine plains and Brazilian parklands, perhaps the only favorable areas in South America for cultures primarily following a hunting economy (Bennett and Bird 1949); second, the fact that there are few specific resemblances to Early Lithic traditions in North America, and those that do appear are the projectile point forms that date from the postfluted point epoch (Plainview, etc.). The single-notched points of Menghín's Solanense complex, suggestive of the North American Sandia form, are an exception to this. In sum, it appears that meaningful relationships between North, Middle, and South America are as difficult to establish on the Early Lithic as on the later stages of our sequence.

ARCHAIC STAGE

Our use of "Archaic" as a stage designation is not as far from its current meaning as might appear. The term was introduced into Eastern archeology by Ritchie, who applied it to his Lamoka phase in central New York (1932). Soon after, the rich preceramic culture of the northern Alabama and Kentucky shell middens was revealed by the extensive W.P.A. excavations of Major Webb and his associates. Relationships to Lamoka were noted and the term "Archaic" extended accordingly, with a "pattern" significance in the Midwest taxonomy (Webb and DeJarnette 1942). About the same time, re-examination of earlier shell midden investigations in the far Southeast (Wyman 1875; Claflin 1931) and current excavations on the Georgia Coast expanded the Archaic still further geographically and at the same time slanted it toward the concept of a "Shellmound culture." Similar extension was going on all over eastern North America, and there were suggestions that cultures as far away as California were somehow related (Haag 1942; Beardsley 1948). However, it was also becoming clear that the concept had outgrown its specific historical implications, and Griffin (1946) made an attempt to jettison it. Failing in this, he and his co-workers reinstated it as a "period" in what may be called the new Midwestern chronology (Griffin 1952). It is our contention that periods

in area chronologies of wide geographical scope lose their temporal significance and become in effect developmental stages. In using "Archaic" as a stage designation we are, therefore, simply making use of its current implications. This leaves us free to expand the concept to its ultimate hemispherical limits.

As previously noted, our point of departure for the definition of an Archaic stage is the general level of stone technology marked by the addition of grinding and polishing to the earlier techniques of percussion and pressure flaking. This is by no means an adequate definition, however; we shall find that some of the cultures designated as Archaic in this paper are without ground stone forms of any kind. Nevertheless, it is appropriate to begin our definition with this category, which certainly represents a technological innovation of immense significance. Of primary interest as criteria are the heavy woodworking tools, generally accepted as prerequisite to the successful occupation of forest environments—axes, adzes, gouges, etc. Second in importance only to these are implements and utensils used in the preparation of vegetable foods and therefore essential to gathering cultures—milling stones, metates, manos, mortars, pestles, pounders, stone vessels, etc. Other Archaic elements of significance in particular areas are ground slate points and knives; atlatl weights of various forms; plummet-like objects (variously referred to as "plummets," "charmstones," "bolas stones," "fusos"); stone tubes, which may in some cases have been smoking pipes; stone beads; and an array of other objects of problematical or purely ornamental function. Thus the addition of a new way of handling stone resulted in an immense increase in variety and complexity of the archeological inventory.

In the chipped stone department, it is noteworthy that the high standards characteristic of some Early Lithic cultures are not generally maintained. Many of the older forms, however, persisted with little modification, particularly in the chopper and scraper categories. The most useful general criterion is the trend toward stemmed, corner, and side-notched point forms, roughly in that order. In areas where ground stone artifacts are rare or absent, such points are of crucial importance in classification. Another Archaic feature in chipped stone is a marked increase in the number and variety of drills—a matter that is doubtless related to the fact that many of the ground stone forms mentioned above are perforated, and to the great emphasis on beads and pendants of stone, bone, shell, and other materials that is an almost universal feature of Archaic cultures.

We would be able to record a number of Archaic determinants in the "rough" stone category if we had this class of objects under better archeological control. Plain, pitted, and faceted hammer-stones, anvil stones, notched pebbles, saws, abraders, whetstones, polishing stones, etc., are consistently reported from Archaic sites. We know little about the history of most of these forms—some of them probably go back to the Early Lithic—but the association of a number of them together, and in quantity, is a fairly reliable general criterion of the Archaic stage.

Finally, of doubtful status as artifacts, but extremely characteristic of

Archaic sites in the Americas, are masses of fire-cracked stones used in pit roasting and stone boiling. In areas where stones were unobtainable, objects of baked clay were used for the same purpose.

Objects of bone, horn, and ivory, present but not abundant in the Early Lithic stage, partly due, no doubt, to unfavorable factors of preservation, assume a major importance in the artifact assemblages of the Archaic. These materials vie with stone for many implements and ornaments, as points, knives, scrapers, tubes, beads, pendants, but in the main were used for artifacts that have no counterparts in stone. Most important numerically are awls, perforators, and needles generally regarded as adjuncts to the technologies of basketry and work in skins. An equally large number of forms—gorges, leisters, harpoons, fishhooks—testify to the importance of fishing and marine hunting activities, for which there is little previous evidence in the Early Lithic. Shell makes its first appearance as material for artifacts, largely articles of personal adornment, especially beads and pendants, except in certain coastal and island areas where it is used for implements normally made of stone. Other materials that seem to have made their first significant appearance in the Archaic, but only in regions near the sources of supply, are copper and asphaltum. Finally, although the word is often used interchangeably with "preceramic," many of the cultures referred to as Archaic in this paper have a well-developed pottery technology.

The above-listed artifacts are found in village site refuse and with burials, and in a very important sense these conditions are themselves criteria of the Archaic. In suggesting that burials first appear on this level we do not mean to imply that Early Lithic peoples had no formalized modes of disposing of their dead but simply that it is only in the Archaic and later stages that we can say what they were. There is, as might be expected, too much variety to permit generalization on a hemispherical scale, but certain modes and features, such as flexed inhumation in round graves, partial cremation, and the use of red ochre in graves, have immense distributions in the Archaic stage.

The first faint evidence of houses appears in the Archaic, but remarkably little can be said about them. There is some evidence that the pole-supported pit-house was known in western North America in early Archaic times (Harrington 1948b), but the scanty evidence in the East indicates a very insubstantial structure that leaves no post-hole patterns in the ground. Other domestic improvements may be seen in the occasional evidences of storage pits on Archaic sites, but this does not become a general feature until the coming of agriculture.

Archaic settlements are characteristically small in extent, but the depth of deposit often indicates considerable temporal continuity, from which a sedentary, or at least seasonal, type of occupance may be inferred, quite different from the nomadic way of life assumed for the Early Lithic. This, of course, could be made possible only by the development of specialized subsistence economies in favored localities. As an example, it seems to be only in the Archaic that fishing, and especially shell fishing, became important in the

economic picture, an adaptation to coastal and interior waterside environments that is clearly evidenced by the over-all distribution of Archaic cultures. We have already suggested as a prerequisite for this development a knowledge of boats and the tools necessary to build them. Another sharply contrasting example is the widespread seed-gathering economy of the North American Great Basin and Southwest, which also, to a lesser extent perhaps, tended to anchor populations in favored areas and, by conditioning them toward dependence on vegetal rather than animal foods, prepared the way for the acceptance of agriculture at a later time. As the theoretically older hunting pattern became more precarious with changing climatic conditions in the Altithermal period, about 5000 to 2500 B.C., this gathering culture proved to be more viable and became the basis for all future development in the areas concerned.

Since our concept of an Archaic stage is largely based on eastern North American data, we will give initial emphasis to that area. Use of the concept in a broad developmental sense obliges us to call many cultures Archaic that have not been so designated before, but we will leave these aside for the moment and concentrate on those about whose status there can be no disagreement, although perhaps no two Easternists would organize them in the same way. One possibility would be to set up the old "Eastern Archaic" (Ford and Willey 1941) as a major cultural tradition, but our feeling is that too much time and space have been built into this unit and that ultimately a considerable number of separate traditions will have to be formulated within it. A "Southeastern Archaic," preferably with another name, could certainly be organized around the well-documented shell middens of the Tennessee River in northern Alabama (Webb and DeJarnette 1942, 1948a, 1948b, 1948c) and the Green River in Kentucky (Webb 1940, 1946, 1950a, 1950b; Webb and Haag 1939, 1947). Temporal phases of this "Shell Mound" culture will have to be worked out, however, for there is a good possibility that more than one tradition is represented even here. "Southeastern Archaic" is generally understood to include the fiber-tempered pottery "horizons" on the Savannah River (Claflin 1931; Fairbanks 1942; Miller 1949, 1950), Georgia Coast (A. J. Waring, Jr., personal communication), and St. Johns and Indian rivers in Florida (Goggin 1952; Rouse 1951b), but it might turn out to be more sensible to make fiber-tempered pottery the basis for a separate far southeastern tradition. The St. Johns and Indian River phases also show the influence of the very distinctive Glades tradition of lower peninsular Florida, which is not generally called Archaic but certainly remained on an Archaic level throughout most of its span of existence (Goggin 1949).

The extension of Southeastern Archaic from the nuclear Kentucky-Tennessee region eastward up the Tennessee River seems to have been slight (Kneberg 1952). To the north and west it includes the early phases of the long-lived Eva "focus" on the lower Tennessee River (Lewis and Kneberg 1947), perhaps the Faulkner site in southern Illinois (MacNeish 1947; Cole and others 1951) and the prepottery sites near Carbondale (Maxwell 1947, 1951). So far, no closely related phases have been reported from the lower

Mississippi. Tchefuncte (Ford and Quimby 1945) has often been mentioned in this connection, but it would be better to leave the position of this phase in suspension until its relationships with other early cultures in the lower Mississippi have been clarified. How far up the Ohio River the concept of a Southeastern Archaic can be carried is hard to say—probably as far as the extensive but poorly documented shell middens about the Falls of the Ohio at Louisville (Guernsey 1939, 1942).

Chronological estimates for Southeastern Archaic, formerly held within a modest first millennium B.C., have been considerably extended by radiocarbon dating. It now appears that the tradition was well established by 3000 B.C.[7] Its terminal date is less easy to fix, some authorities maintaining that it persisted with little change down to a late prehistoric period (Lewis and Kneberg 1947).

The idea of a "Northeastern Archaic" is sometimes met with but does not seem to have found favor with the archeologists of that area. The pioneer Archaic phase, the Lamoka of central New York (Ritchie 1932), is still rather isolated so far as direct relationships are concerned. It also remains the earliest manifestation of the Archaic in the Northeast, with a radiocarbon date of 3433 ± 250 B.C. (Sample No. C-367, Libby 1952a) which compares very closely with the Southeastern Archaic dates referred to above. A broader, if somewhat insubstantial, formulation is the later Laurentian (Ritchie 1944), to which the fluid concept of tradition would seem to be ideally suited. As the name implies, Laurentian is thought to be intrusive into New York from the St. Lawrence Valley (Ritchie 1951a) and also over into New England, where it incorporates the famous Red Paint phase of coastal Maine (Willoughby 1898; Moorehead 1922b; Smith 1948). To the west it has relationships of an uncertain character with the Old Copper culture of the Upper Great Lakes region[8] (Miles 1951). Laurentian is also of interest because of a special flavor contributed by ground stone gouges, rubbed slate points, and semilunar knives, formerly attributed to Eskimoan influence but now thought of as relating to an ancient circumpolar distribution (Gjessing 1944; Spaulding 1946; Ritchie 1951b).

A coastal "substratum" extending from southern New England down through the Middle Atlantic states, is a possible point of departure for another Archaic tradition (MacNeish 1952). The existence of a nonceramic culture was demonstrated at the Crispin site near Trenton, New Jersey, in one of the earliest systematic stratigraphic excavations in North America (Hawkes and Linton 1916), but because earlier claims of a "paleolithic" technology at the famous Abbott Farm site in the same locality (Abbott 1872; Volk 1911) had overshot the mark, there was a great reluctance to accept even an Archaic status for the culture. Subsequent investigations at the Abbott Farm and related sites appeared to support the skeptics (Cross 1941, 1943), but it has been suggested that the methods of excavation obscured the stratigraphy (Schmitt 1952:60). Whether a tradition based on this abundant and well-known New Jersey material can be extended northward to include Archaic

sites in the Long Island region (Smith 1950) and Massachusetts (Johnson and Raup 1947; Bullen 1949), and how it relates to the coastal phases of Southeastern Archaic in Georgia and the Carolinas, are questions beyond the scope of this paper.

There may be another Archaic tradition in the Northeast, based on the taiga of northern New England, the Maritime Provinces, Newfoundland, and southern Labrador, an environment in which culture remained in an Archaic stage down to the ethnographic present, but the idea is still in an unformulated state (information from Frederick Johnson).

The Eastern cultures so far considered can be listed as Archaic without raising any eyebrows. There are other cultures in the area, however, that we are compelled by the logic of our definitions to relegate to this stage, about which there might be some dispute. Lack of space forces us to hypothesize at this point in a rather sweeping fashion. As a vehicle for generalization we may use the current Midwestern chronology (Griffin 1952a). This scheme, in brief, classifies Eastern cultures into a series of "periods," which are at the same time large-scale cultural units, as follows: Paleo-Indian, Archaic, Early Woodland, Middle Woodland, and Mississippi (which includes Late Woodland). We start with the assumption that the basic adaptation to the eastern forest and waterside environments is represented by the various Archaic traditions already considered and that their ultimate sources lie in the boreal cultures of the Eurasiatic mesolithic and neolithic periods. Upon this substratum, we maintain, was grafted one or several pottery traditions, also of north Eurasiatic origin, resulting in the Early Woodland cultures of the Midwestern scheme. Whether the same northern influences were responsible for the introduction of the earliest burial mounds is not yet clear. The incorporation of pottery (and possibly burial mounds) did not bring about any changes of significance from a developmental point of view. For the most part these "Early Woodland" cultures must be regarded as Archaic in terms of the definitions used here.[9] However, we have to consider the possibility that agriculture, already present in northeastern Mexico and the Southwest for a long time, was beginning to percolate into the East in Early Woodland times, and this would, by definition, shift cultures in which such evidence might be found into the Preformative. The Middle Woodland period is marked by the rise of a number of traditions, mainly from local sources but with outside contributions the origins of which are still obscure. We assume that influences from the Eurasiatic north were still active, but there are now influences from the south to be reckoned with, and these are of overriding importance for us because now for sure they are involved with the spread of agriculture and associated elements. We assume, usually without concrete evidence, that agriculture is present in all Middle Woodland cultures and assign them *en bloc* to the Preformative stage. Wherever Middle Woodland came into contact with pre-existing Archaic and Early Woodland cultures, they dominated in the resulting mixture, but on the northern and eastern peripheries this dominance was not permanent and there followed a re-emergence of the simpler Woodland

patterns, the "Late Woodland" of the Midwestern scheme. This late regressive situation is, of course, complicated by influences from Formative stage cultures, which by this time are flourishing to the south. Beyond this uncertain and probably fluctuating borderland, which is in effect the northern and eastern limits of successful maize agriculture, are the vast forest areas in which culture remained on an Archaic level down to the ethnographic present.

The above grossly oversimplified version of the cultural succession in the East is prerequisite to an understanding of what happened in the Plains. The concept of a "Plains Archaic" has found expression in the literature, but the cultures so designated are difficult to fit into our developmental scheme. The best documented phase, Signal Butte, in western Nebraska (Strong 1935), shows a persistence of lanceolate point types of generalized Early Lithic character in a context that includes stemmed and notched forms, drills, and occasional ground stone implements. The general characteristics of the site and deposits indicate a bison-hunting economy little changed from Early Lithic times. The single available radiocarbon date is well within the time range of Eastern Archaic cultures.[10] The same general observations apply to the Frontier "complex" in the Medicine Creek Reservoir region of south central Nebraska (Holder and Wike 1949). The deeply buried Lime Creek sites in the same locality (Schultz and Frankforter 1948; Davis 1953) have also been called Archaic, but the points are more definitely Early Lithic in type (Plainview and Eden-Scottsbluff) and geological estimates and radiocarbon dates are early.[11] It seems almost necessary to assume that the Archaic type materials on these two sites are later than the Early Lithic points, but the stratigraphy has not yet been published in detail.

In the eastern Plains and prairie borderlands lanceolate points continue to predominate in "Plains Archaic" cultures but there is a greater mixture with typical Eastern Archaic forms. The Nebo Hill phase near Kansas City, for example, has points that would pass for Signal Butte or Plainview in a context that includes grooved stone axes (Shippee 1948). Further east, in central Missouri, the distinctive trianguloid Dalton point is associated with lanceolate points of the same general character (Chapman 1948). East of the Mississippi, the "Starved Rock Archaic" from the lower levels of the famous table rock site on the Illinois River seems to be more closely related to Plains Archaic than to other Archaic traditions in the East (Mayer-Oakes 1951).

It would seem to follow that, if Plains Archaic is typologically nearer the ancestral Early Lithic culture, it may be earlier than Eastern Archaic. Indications that such is the case may be seen at Graham Cave, in central Missouri, where the transition from Plains Archaic to Eastern Archaic is demonstrated stratigraphically (Logan 1952). In the Ozarks of northeastern Oklahoma, the three sequent phases of the Grove "focus" reflect the same transition (Baerreis 1951).

It must be acknowledged that these Plains Archaic cultures pose a kind of dilemma that is perhaps unavoidable in a developmental scheme that also attempts to maintain contact with history. A current theoretical reconstruc-

tion of prehistory in the Plains holds that the dry Altithermal period produced conditions generally unfavorable to hunting; the Pleistocene mammals disappeared and the country became practically uninhabitable to man on an Early Lithic stage of cultural development. With the return of favorable conditions, and great herds of bison, in the Medithermal, man again occupied the Plains, but with a different culture, a larger range of implements, some of which were comparable to Eastern Archaic forms (Krieger 1953). It might be added that said additions and modifications applied mainly to elements that could be incorporated into a grassland hunting type of economy. Thus an Archaic flavor was acquired, but in a more fundamental sense the cultures remained on an Early Lithic level. Nevertheless, it does less violence to accepted views to call them Archaic, and this is the horn of the dilemma we have chosen.

The diffusion of Woodland pottery into the Plains, though belated—it is generally assumed to have taken place on a Middle Woodland time level—does not seem to have marked any significant advance in cultural development. The very large number of sites of the Valley phase indicates a fairly intense and widespread occupation of the stream valleys at this time (Hill and Kivett 1940; Smith 1949), but their small size and meager deposits suggest a continuation of the migratory hunting and gathering economy of the Archaic. The distinctive Sterns Creek phase of eastern Nebraska, however, also designated as Woodland of the same "Middle" period, furnished evidence at the type Walker-Gilmore site of squash and gourds (but no corn and beans), which would put it into the Preformative (Sterns 1915; Strong 1935; Champe 1946). This has been interpreted as part of the evidence for an incipient "pre-Central American" agriculture (Carter 1945), but there is nothing in the stratigraphic picture at Walker-Gilmore, complicated as it is, nor in the general archeological probabilities, that would give the slightest support to such a theory.

Identification of an Archaic stage in Texas is subject to the same difficulties as in the Plains, owing to the persistence in much of the area of conditions favoring an Early Lithic type of existence and to the almost complete lack of polished stone forms in the archeology. However, it is an extremely important area, from our point of view, as a much-needed bridge between the Archaic cultures of the East and the so-called desert cultures, which we are about to designate as Archaic in the Southwest. A very large part of Texas archeology is involved. Early Lithic finds, generally of Folsom-Yuma type, have been mostly confined to the central and western portions of the state. Preformative cultures, on the other hand, except in northeastern Texas, which is better considered as part of the greater Southeast, are also limited to the west, where they appear to stand in a late marginal relationship to the Southwest. Most of the central and coastal regions of Texas are without even this late marginal type of Preformative culture.

This leaves a vast amount of archeology that we would call Archaic. Most of it has been lumped into a Balcones "phase" in the McKern taxonomic

sense[12] (Stephenson 1950). The validity of this formulation has been attacked (Krieger 1951) and defended (Kelley 1951a). For present purposes it seems preferable to cut it back to something like its original connotation, which was, in effect, a combination of the Edwards Plateau and Big Bend aspects or, as we would prefer to call them, traditions. The former, located in central Texas, is known from open camp sites and "burned rock middens" yielding a typically Archaic chipped stone assemblage and reflecting a migratory hunting-gathering type of culture. Of the three "foci" of this tradition, the early Clear Fork (Kelley 1947a), characterized by certain triangular and stemmed point types and a specialized end-scraper or "gouge," has long been the center of a dating controversy, with estimates running as early as 8000 B.C. or earlier. General typological considerations favor the more recent estimates of 4000 or 2000 B.C. (depending on certain alternative geological correlations) to about A.D. 1500 (Kelley 1947a). The Round Rock focus (Campbell 1948) is thought to have been approximately coeval, but the Uvalde focus (Kelley 1947b) occupied only the last two or three centuries of this long time span.

The Big Bend tradition of the trans-Pecos region in the extreme western part of the state is known from rock shelters as well as open sites and has in consequence a larger cultural inventory, including many perishable items, and a more Southwestern flavor (Kelley, Campbell and Lehmer 1940). The early Maravillas focus of this tradition has been compared with Ventana-Amargosa I of south central Arizona; the intermediate Pecos River focus has Gypsum type points, and the later Chisos focus has Pinto and San Pedro types (Kelley 1951b). Otherwise the stone assemblages compare closely with those of the Edwards Plateau tradition and the time range is thought to be similar, except that the terminus of the Big Bend tradition may be slightly earlier. Two of the later foci of the Big Bend aspect, however, Chisos and Livermore, have furnished evidence of agriculture, so from our point of view the Archaic ends in this region considerably earlier than anywhere else in Texas. In view of the recent early dates for corn in Tamaulipas and New Mexico, to be discussed in a later section, it will not be surprising if the Preformative threshold is pushed further back in future investigations in this region.

Generalizations about the Great Basin and Southwest on the Archaic level involve a continuation of difficulties encountered in the Early Lithic section. These have to do specifically with that great continuum of basin and range "gathering" or "desert" cultures which, because of the importance of milling stones and metates in the archeology, are so often contrasted with the "early hunters" in the literature. We chose to play down this dichotomy between hunters and gatherers and, without really committing ourselves, suggested that the earliest phases of said continuum might be classified as Early Lithic. To make matters more complicated, its later phases are involved in another dichotomy, that of gatherers versus farmers (Kirchhoff 1954). For example, the continuity of the Cochise tradition, centering in southeastern Arizona and southwestern New Mexico, has been emphasized from the very outset (Sayles and Antevs 1941), and if we consider only the stone artifacts

it appears to be undeniable. However, Sulphur Spring is a hunting-collecting phase, which looks more Early Lithic than Archaic in terms of the criteria used here, whereas Chiricahua and San Pedro, in some localities at least, are agricultural phases and therefore Preformative, although there is evidence of a renewed importance of hunting in the latter (Campbell and Ellis 1952). Thus from our point of view Cochise passes directly from Early Lithic to Preformative without any intervening Archaic stage. There is no reason this should not happen in this kind of classification, but the problem is complicated by uncertainties of dating. The earlier overgenerous estimates for the various phases of Cochise, based on geological correlations, have not been confirmed by Carbon 14. A round average of two discrepant dates for Sulphur Spring of 5000 B.C. compared with about 2000 B.C. for Chiricahua[13] leaves a gap approximately equivalent to estimates for the Altithermal, which suggests that the continuity of Cochise may have been overemphasized. If so, our choice of leaving Sulphur Spring in the Early Lithic is not unreasonable, and the absence of an Archaic stage may not be final.

In the west, in the lower Colorado River and California desert regions, the Amargosa tradition parallels Cochise but with some important differences. There is no Early Lithic phase, apparently. The early Malpais or San Dieguito I has no grinding stones and is not considered as part of the gathering continuum under consideration here. Amargosa II, or Pinto-Gypsum as it was formerly called (Rogers 1939), correlates roughly with Chiricahua Cochise in time (Haury and others 1950:534) but shows no sign of agriculture and not much of collecting; hunting is still (or again?) the dominant mode. Stone types are Archaic in character. Whether or not it is correct to equate Gypsum and Pinto is a question we do not have to go into. If point typology means anything, both Gypsum and Pinto points are widespread horizon markers on an Archaic level. We have already referred to the place of Pinto points in Texas Archaic. The same comment could be extended to cover most of the eastern United States. The distribution of Gypsum-like points is hardly less wide (Hurt 1953:215). How this can be reconciled with the extinct faunal associations at Gypsum Cave (Harrington 1933) and the very early radiocarbon dates furnished by sloth dung in the cave (8504 ± 340 B.C. and 6576 ± 250 B.C., Sample Nos. C-221 and C-222, Libby 1952a) is difficult to see. This is hazardous ground for the nonspecialist, but the evidence seems to be accumulating against any possibility that these dates are correct or, if they are, that they date the Gypsum culture.

Getting back to the Amargosa tradition, stage III, equating roughly with San Pedro Cochise, with hafted drills, perforated slate ornaments, and stone tubes, is highly reminiscent of late Archaic stage cultures in the East.

At Ventana Cave, in south central Arizona, an intermediate geographical position, Amargosa and Cochise traditions mingled in an excellent stratigraphic sequence (Haury and others 1950). Just above the discontinuity that is believed to mark a long period of erosion and abandonment of the cave was a thin deposit containing artifacts of the Amargosa I phase, previously postu-

lated but not isolated in the lower Colorado. The characteristic broad stemmed points, somewhat similar to Borax Lake points (Harrington 1948a), are well within the range of Eastern Archaic forms. This was followed by thick midden deposits, the lower portion containing both Amargosa and Cochise types, called Chiricahua-Amargosa II, the upper being San Pedro Cochise. The upper terminus of the midden was marked by another disconformity, followed by Hohokam of the Colonial and Sedentary periods. There was no sign of agriculture in the Chiricahua-Amargosa II and San Pedro deposits, so from our point of view these phases remain in the Archaic stage. Haury points out that in the meeting between Amargosa and Cochise traditions it was the latter that proved to be stronger. Cochise is therefore of transcendant importance as the basis for all three of the dominant cultural traditions of the Southwest.

A number of other cultures would have to be considered in any complete description of the Archaic in the Southwest, a few of which can be briefly mentioned. Such, for example, is the San Jose culture of northwestern New Mexico, found in fossil sand dunes dating from the Altithermal period (Bryan and Toulouse 1943; Bryan and McCann 1943). Closely related to San Jose is the Concho "complex" from undated surface sites about a hundred miles away in east central Arizona (Wendorf and Thomas 1951). Both these cultures are related in a general way to Pinto-Gypsum but appear to show a greater dependence on hunting. The over-all character of the artifacts, and many of the specific forms, are well within the range of Archaic as defined in this paper.

In sum, the general Southwestern picture in the Archaic is one of a basic uniformity, possibly imposed by over-all environmental homogeneity with collecting as the most characteristic economic pattern, but with marked local and temporal variations in the role of hunting, and, in the eastern portions, an early shift to agriculture and a Preformative status, unaccompanied by any recognizable changes in the artifact inventory.

An impressive accumulation of finds in the central and northern Great Basin relate to another, perhaps even more fundamental, small-game-hunting–collecting continuum for which the name "Bonneville" has been suggested (Jennings 1953:208). This extremely long-lived and inclusive tradition, intended only to serve as a preliminary formulation, will undoubtedly be subdivided, but it is very useful for our present purposes since all the cultures involved appear to be on an Archaic level. These include Deadman (Smith 1941, 1952), Promontory and Black Rock (Steward 1937, 1940), and Danger (Jennings 1953) caves in the Great Salt Lake region; Lovelock Cave (Loud and Harrington 1929) and Leonard Rock Shelter (Heizer 1951) in the Humboldt Sink region of western Nevada; and the early Oregon manifestations (Cressman 1942, 1951). General typological affiliations are with the Amargosa tradition already discussed, but the remarkably early radiocarbon dates for some of these cultures, running as high as 9500 B.C. at Danger Cave,[14] raise some very serious problems. Until the discrepancies between some of these dates and the local geological correlations are resolved, and until the temporal phases of this "long and placid" tradition have been worked out and described, we

may be excused for not coming to grips with the classificatory problem. We can simply record the possibility that this Bonneville tradition was already in existence in a period occupied by Early Lithic cultures elsewhere and, if so, was probably basic to all the Archaic cultures of the Greater Southwest. Whether this would negate the assumption of temporal priority of Early Lithic over Archaic stage cultures in western North America is a question that can be neither settled nor ignored. We have already referred to certain eastern Archaic dates that pose the same problem.

Among the best-documented Archaic stage cultures of North America are those of the Sacramento-San Joaquin Delta and San Francisco Bay regions of central California (Schenck 1926; Gifford and Schenck 1926; Schenck and Dawson 1929; Heizer 1949). Without subscribing to the view formerly held that nothing exciting ever happened in California prehistory, it seems that the development summed up in the Early-Middle-Late "horizon" sequence in these regions is one of successive elaboration of the same basic elements entirely within the Archaic stage as conceived in this paper. Relationships to Eastern Archaic have been frequently pointed out (Haag 1942; Beardsley 1948), but such discussions usually have had to do with the possibility of culture-historical connections, which is not germane to our present purpose. The relationships from the point of view of culture-developmental stage are indubitable, whatever the cause may have been. According to Beardsley, the elements that support the theory of an actual historical connection are mainly in the Early and Middle horizons. The Late horizon, particularly in its climax Cosumnes phase (Lillard and Purves 1936), shows a density of population and relatively advanced level of culture made possible by a stabilized food supply in the form of a highly specialized acorn complex. This might be regarded as an alternative to agriculture in its capacity to produce conditions for a Preformative or even Formative stage, but other requisites are lacking so, with some reservations, we would likewise consign this horizon to the Archaic.

Without going into the complicated question of the correlation between central California and the Santa Barbara region, we can briefly summarize the latter, in terms of developmental stage, as follows. The earliest Oak Grove phase (Rogers 1939) is not a complete cultural unit, but the succeeding "Hunting People," even as described in the original report, is definitely Archaic. The following Canalino phases (Orr 1943, 1952a), which seem to equate roughly with the three horizons of the central California continuum, are equally typical of Archaic stage culture. The final Canalino phase has been identified with the historic Chumash occupation of the region. Village populations, according to estimates of the early discoverers, ran up to one thousand people and eighty houses (Orr 1943), a pattern a good deal more intensive than we think of as characteristic of the Archaic, but, like the Cosumnes in central California, this is a climax phase in relation to California as a whole.

A similar problem arises in connection with the Northwest Pacific Coast. To consign all the archeology of an area that was the scene of one of the most intense cultural climaxes in the New World to the Archaic stage does violence

to our sense of values, but examination of the archeology leaves us no alternative. If we can take the recently documented Fraser Delta sequence (Borden 1951) as typical for the southern Northwest Coast—the archeology of the northern portion is not well known—there was an "Early" phase with a profusion of ground slate forms and other elements suggestive of an Eskimo sea-mammal hunting culture; followed by an "Intermediate" phase with antler wedges, stone hammers, and adze blades, all testifying to a well-developed woodworking industry of presumed interior origins; and, finally, in the late prehistoric and early historic periods, a blend of these coastal and interior features. In spite of the fact that this final phase is identifiable with the Coast Salish in what Kroeber regarded as the original center of historic Northwest Coast culture (Kroeber 1939:31), the remains that survive in the form of archeology are such as would be unhesitatingly classified as Archaic if found anywhere else. This is also the period in which occurred the spread of Asiatic traits which gave Northwest Coast culture its most characteristic flavor, but these are for the most part such as to leave no traces whatever in the ground (De Laguna 1947:12).

It must be admitted that the consignment of all Northwest Coast culture, past and present, to an Archaic stage is not going to contribute any insights into the problems of that area. The sad fact is that our definitions have only nominal value in peripheral situations, since they are based very largely on developments in or near the center. This is particularly true in the case of the Northwest Coast, in which there are good reasons to believe that Asiatic and possibly Oceanic influences played a more decisive role than those from more southerly parts of the North American continent.

We have already pointed out the difficulty of applying our classificatory criteria to cultural developments in the Arctic. Here we are dealing with cultural traditions so unique in total configuration as to make comparisons appear a trifle ridiculous. Nevertheless, with the possible exception of the Denbigh complex, as already noted, it is surprising how many of the criteria of our definition of Archaic are applicable to Eskimo cultures of the far north. This is particularly noticeable in the Dorset tradition of the eastern Arctic (Jenness 1930; Rowley 1940; Leechman 1943; Harp n.d.), whose relationship with northeastern Archaic is a perpetual subject of discussion (Hoffman 1952). But even such climactic Eskimo phases as Old Bering Sea (Collins 1937) and Ipiutak (Larsen and Rainey 1948) would, if subjected to the same unfavorable conditions for preservation, appear to be on the same general level of development as the well-known Archaic cultures further south. There is not only a basic conformity in stone and bone technologies but a great many shared elements of specific nature. Until fairly recently the tendency was to attribute the presence of such traits in Archaic contexts to "Eskimoan" influence. Now, with the far greater time depth allowed to North American Archaic cultures, the problem of interchange between them and contemporaneous Eskimo cultures has been immensely complicated, and it would be a rash student who would attempt to disentangle them.

The case for an Archaic stage in Mexico and Middle America is similar to

that outlined for the Southwestern and Great Basin areas of the United States. The polished stone industries of the eastern United States or California Archaic are lacking; nevertheless, cultures with projectile point forms and ground stone tools similar to those of other Archaic manifestations occur in Middle American sequences. In Tamaulipas, the Nogales phase, which follows the Early Lithic Diablo and Lerma phases, is characterized by tear-drop and triangular shaped points, large oval blades, chipped stone hoes or celts, and frequent crude hammerstones, manos, and metates (MacNeish 1950:92). A subsequent development of the Nogales phase, signaled among other things by the appearance of maize, resulted in the Preformative La Perra phase. In this chronological and cultural position there is a strong argument for placing Nogales as Archaic. By extension, the preceramic Chalco complex of the Valley of Mexico is assigned to the Archaic upon the basis of its artifact similarities to Nogales (MacNeish 1950:87). Quite probably, the Abasolo and Repelo complexes of the Tamaulipas coast fit into the Archaic bracket (MacNeish 1947). In Coahuila, the broad triangular projectile points, beads, and shell ornaments of the cave cultures suggest the Archaic (Martinez del Rio 1953).

On the Chiapas coast, at Islona de Chantuto, the preceramic levels of shell middens have yielded little in the way of distinctive artifacts (Drucker 1948) and may, possibly, pertain to anything from Early Lithic to Preformative; however, the nature of the site deposit suggests a rating of Archaic or later.

Farther south, on the Pacific side of Panama, shell middens on abandoned shorelines indicate an old fishing and shellfish gathering population for that region which antedates the later, definitely agricultural, and Formative, cultures. Two phases of this littoral zone occupation have been defined, the Monagrillo and the Sarigua (Willey 1951a; Willey and McGimsey 1952, 1954). The Monagrillo is characterized by crude pebble grinding and chopping tools, crude mortars, chipped stone scrapers but not points, and simple plain or incised and punctated pottery. Sarigua, which may be later than Monagrillo, lacks the stone chopping and grinding tools but possesses pottery of a simple kind though in a style quite different from Monagrillo. The location of Monagrillo and Sarigua sites on abandoned shore lines and lagoons, and the nature of their refuse, argue against the presence of agriculture; and, in spite of the presence of pottery, their over-all configuration is that of the Archaic.

A somewhat comparable situation is reported by Reichel-Dolmatoff (1954) with reference to the Isla de los Indios culture of the swamp lands of the Lower Magdalena River in northern Colombia. Apparently the earliest archeological phase in the region, and manifested in sites in terrain that is unfavorable to agriculture, the Isla de los Indios phase has an Archaic cast. It is a ceramic complex, featuring incised and rocker-stamped vessels of rather simple form. In this connection, it should be noted that a rocker-stamped technique of pottery decoration characterizes the early Formative stage cultures of Peru and middle Formative stage cultures of Middle America. The same technique is, of course, the hallmark of North American Hopewellian phases, which we have classed as Preformative.

In Peru, the best-known preceramic deposits are those of Huaca Prieta and Cerro Prieto cultures, which we discuss below as Preformative. However, an extensive nonpottery site has been reported from the far north coast, some twenty-two miles inland from Cabo Blanco. At this station a large collection of artifacts includes some ground and polished stone pieces as well as a great number of coarse unifacial implements (Brown 1926). It is, of course, possible that this site represents a preceramic, agricultural culture comparable to the Preformative Huaca Prieta.

At Arica and Pichalo, in northern Chile, Archaic cultures are well represented by two sequent periods or phases. Shell fishhooks, cigar-shaped stone sinkers, composite stone fishhooks of stone and bone, stone mortars, and stone bowls are among the distinctive artifact types (Bird 1943). Rough percussion flaked artifacts are found all along the Chilean coast, but these are clearly associated and contemporaneous with pressure-flaked and ground stone implements. In brief, there is no evidence of Early Lithic type cultures antedating those of the Archaic along the Chilean littoral (Bird 1943). At both Arica and Pichalo maize agriculture and ceramics follow the Archaic periods. In the Arica sequence there is some slight indication that maize may have preceded pottery, but this is not certain.

To return to Bird's (1938) cultural sequence in the far south, it will be recalled that periods 1 and 2 of this chronology—as well as other presumably early cultural complexes of Tierra del Fuego, Patagonia, and the Pampas—were classed as Early Lithic. New types of projectile points and the common presence of the bolas stone in periods 3, 4, and 5 are accepted as markers of the Archaic stage. Cultures of this Archaic type prevailed throughout Patagonia, the Pampas, and along the lower Paraná River until historic times. As the Magellanic evidence indicates, the earlier periods of this Archaic development of southern South America were preceramic. No approximate date for the first appearance of pottery in these areas has been hazarded. There is no doubt that the trait spread in prehistoric times from north to south, probably diffusing from foci on the lower Paraná River. Arroyo Sarandí and El Cerrillo (Lothrop 1932), near the delta of the Plate, are representative of these later, pottery-using Archaic cultures in the northern Pampas. Bone implements and objects, chipped and ground stone, and incised and punctated pottery of simple forms are included in the assemblages. Life seems to have varied between nomadism in the open flat country and relatively stable village occupancy along the rivers. Land game and riverine foods were the staples (Howard and Willey 1948). Only in very late times, with the occupation of sites like Arroyo Malo in the Plate Delta by Guaraní Indians, was an agricultural pattern tentatively established in the area.

Brazilian archeology of both the eastern interior uplands and the Atlantic coast has been almost wholly concerned with cultures of an Archaic level. Although claims of great antiquity have been made for some of the cave finds near Lagoa Santa, the artifactual remains, putatively associated with Lagoa Santa Man, are chipped and ground stone celts, hand stones, pitted anvils, and bone projectile points. Apparently, pottery vessels and pottery pipes

were introduced into these cave sites at a later period. Serrano (1940a, 1946) defines an early coastal shell mound, or *sambaquí*, culture as having an artifact complex very similar to that of the Lagoa Santa caves. Later sambaquí phases (the "Middle" and the "Southern") (Serrano 1940b) are characterized by numerous ground and polished stone artifacts, including plummet-like stones ("fusos"), bolas, mortars, axes, celts, and elaborate dish-shaped zoomorphic forms. Pottery seems to be associated with these complexes, although in some instances it may refer only to later periods of shell mound occupancy. The general chronological range of these eastern Brazilian Archaic cultures is unknown. Historic, and even modern, tribes in the interior have material culture inventories which would, under archeological conditions, appear much as some of the sambaquí assemblages. Thus, there seems little doubt that some Archaic phases existed until historic times. At the other end of the scale it seems reasonable to assume that east Brazilian Archaic cultures may have had their beginnings two, or three, or even four millennia before the opening of the Christian Era.

An Archaic level complex is reported from Manicuare (Rouse 1951a) in Venezuela, and this unit has some similarities to Archaic phases in the West Indies as revealed in such simple tools and artifacts as conch shell gouges, cups, and hammers. Aside from these items, however, the various West Indian Archaic cultures have little in common with the South American mainland. To the north, there are similar, vague analogies in shell implements between the West Indian Archaic and some of the Florida Glades archeological cultures (Rouse 1949). Other than this, the early horizon in the Antilles is an entity without close relationships, and, within itself, there are marked differences in Archaic complexes from island to island (Alegria, Nicholson and Willey ms.). The Couri and Bay of Conch phases of Haiti display polished peg-shaped ceremonial stones, hammerstones and anvils, stone dishes, and ground stone axes. In addition to these traits, the Haitian Archaic also has large unifacial flake knives of a unique kind (Rouse 1941). Cuba shows relationships to Haiti only in the peg-shaped ceremonial stones and in crude hammer and anvil stones (Osgood 1942). For the most part, the Cuban Archaic cultures emphasize the shell tools and dishes which are not characteristic of Haiti. Archaic artifact complexes from Puerto Rico and elsewhere are extremely simple and scanty in content or have not yet been described (Alegria, Nicholson and Willey ms.).

To briefly summarize, the Middle and South American cultures classed as Archaic in this discussion represent numerous historical lines whose origins and connections have not, or cannot, be traced. In Mexico, there appear to be connections with early cultures in the southwestern United States and in Texas. The Chilean coast, another principal center of Archaic type developments, shows an amazing number of parallels to the Archaic units of the southern California coast (Bennyhoff 1950; Lathrap ms.). In the far south, what we have designated as Archaic seems to have deep roots in the old Patagonian and Fuegian hunting cultures of the Early Lithic, although in its

later periods it undoubtedly assimilated various traits, including pottery, from the areas to the north. In eastern Brazil, as well as in the northern Pampas and Paraná River section, there are a number of parallels to the Archaic of the eastern United States (Baerreis 1950; Willey 1951b), and the possible historical significance of these parallels should be pursued. Archaic cultures in the Antilles, which could have conveniently formed a link between those of the eastern United States and those of Brazil, are rather distinctive and highly localized, although there are some similarities both with Venezuela and with Florida.

PREFORMATIVE STAGE

In setting up a Preformative stage between the Archaic and Formative, we are impelled more by theoretical considerations than by exigencies of the archeological record. In theory, there must have been a stage in which the elements of the Formative were assembling, a stage of emergence in the full sense of the word. Naturally, the elements in question—agriculture, stable settlement patterns, craft specialization, and ceremonialism—were not all assembling in the same forms and the same places at the same time. It is not going to be easy to present a coherent picture of such a stage nor to hold it together archeologically, but, as a means of focusing on one of the most important problem areas in American archeology, we deem it worth the effort.

The relationship of the Preformative to the precedent Archaic is, in most regions, the point of greatest difficulty. Given a satisfactory sequence, it is usually possible to say at what point the culture attains a Formative level. But to say where it begins to be Preformative is quite another matter. The indispensable criterion is agriculture, and it is hardly necessary to point out that evidence for agriculture depends on favorable conditions for preservation that are seldom afforded. Nevertheless recent discoveries in widely separated areas have established the presence of domesticated food plants in early cultures that would otherwise be classified as Archaic. We can visualize the lower limits of the Preformative as a very elastic border that is constantly being pushed down by new discoveries into the Archaic. The effect has been to substitute for the picture of a sudden and sweeping cultural "revolution" that of a long period of slow transition, in which the assumed effectiveness of agriculture as an agent of cultural change is not conspicuously evident.

We have said that agriculture is the primary criterion, but it is the incompleteness of its role that is determinant. The Preformative may be characterized as the stage when it is merely another, and perhaps not even the most important, food resource. Hunting and collecting are still followed, with, almost certainly, the greatest emphasis on the second. It is even theoretically possible that collecting, rather than agriculture, was the effective stabilizer; sedentary life may have been a prerequisite for agriculture rather than the other way around. Nevertheless we can assume that, once begun, agriculture's role steadily—if slowly—increased, until, the environment permitting, it

became not merely the dominant economic mode but the central theme of the whole culture. When that point is reached, the culture in question can no longer be called Preformative.

Apart from agriculture, there are no reliable technological criteria. Earlier assumptions of a functional relationship between agriculture and pottery have been abandoned. In some areas agriculture seems to have preceded pottery by many centuries; in others there are well-developed pottery technologies without evidence of associated agriculture. "Digging" implements and grinding utensils are of extremely dubious value as criteria for the Preformative, because both are equally, if not more, necessary in a collecting economy. The consensus of much uncertain evidence is that early agricultural techniques in the New World involved no recognizable new implements or utensils of production or preparation.

Up to this point we have been talking about the Preformative in the sense of an emergent stage leading (not inevitably) to the Formative. We also have to class as Preformative many cultures that stand in a different relationship to the Formative. Such are cultures into which Formative elements have diffused, but through environmental or other limitations they have remained on a Preformative level. This is the standard situation along the climatic and physiographic limits of successful agriculture. Another troublesome, but less frequent, situation is that in which Formative societies have moved into less favorable regions and dropped back into a Preformative stage. It might be useful to refer to these situations as marginal or recessive Formative, but only confusion would result from attempting to set up a separate stage to take care of them.

The only area in which the Preformative can be seen in a context of continuous development from the earlier Archaic to the succeeding Formative is in the North American Southwest. Here, in the region occupied by one of the three major Southwestern cultural traditions, the Mogollon, recent cave excavations have afforded ample evidence of primitive maize—and, later, beans and squash—associated with a lithic culture that differs little from Chiricahua, a phase of the Cochise tradition that we have already classified as Archaic. The first, and apparently earliest, such evidence comes from Bat Cave, in the San Augustin Plains of west central New Mexico. Continuity from Cochise-with-agriculture to Mogollon is suggested in the preliminary account of the excavation (Dick 1952). The corn, which has been intensely studied, shows a slow but progressive development that could only have been the result of careful domestication (Mangelsdorf and Smith 1949). Radiocarbon dates on charcoal from cornbearing levels in the cave run as early as 3981 ± 310 B.C. (Sample No. C-573, Libby 1952a), but the precise nature of the association of the corn and charcoal has not yet been published. The earliest Bat Cave corn is exceedingly primitive; it cannot yet be said with certainty that it is not a wild variety. Reliable evidence of domestication, however, comes early in the sequence. If an extrapolation from the Carbon 14 dates is to be trusted, we can infer that domestication was in progress on the Plains of San Augustin

by about 3000 B.C.[15] The same story of evolving corn in a slowly developing culture is seen in the Tularosa Cave about twenty-five miles northwest of Bat Cave (Martin and others 1952). Here a similar continuity from Chiricahua into Mogollon, all on a Preformative level, can be seen until, with the appearance of architectural innovations and black-on-white pottery in the Three Circle phase, about A.D. 900 we can begin to speak of a Formative stage. Putting these two cave sequences together gives a total span for the Cochise-Mogollon Preformative of some 4,000 years—a very slow-footed revolution!

Nothing comparable to this long-lived "early agricultural" period is found in the Anasazi tradition of the Four Corners region to the north. Here the earliest known culture that could be classified as Preformative, Basket Maker II (Kidder and Guernsey 1919; Guernsey 1931; Amsden 1949; Morris and Burgh 1954) dates from about the first century A.D.[16] The nomadic hunting and seed-gathering Basket Maker I phase, postulated when the Pecos classification was set up in 1927, would be Archaic in our terminology, but it has not yet materialized. Preagricultural sites have been found in the Anasazi area, as we have already pointed out, but they do not correspond to what was expected of Basket Maker I. The origin of Basket Maker II, therefore, is still an open question, and on present showing the solution appears to lie outside the Anasazi area.

Agriculture is well developed in Basket Maker II—there is nothing comparable to the primitive corn of Bat Cave; nevertheless, it does not seem to have become the central theme of the culture, perhaps was not even the most important food resource. Hunting and collecting are still much in evidence. Settlements are at best small unorganized groups of very crude unstandardized surface or shallow pit-houses of "brush or timber masonry" construction and slab-lined storage cists. There are as yet no specialized religious structures or *kivas*, and in general only the faintest indications of developing ceremonialism. Thanks to favorable conditions for preservation in some regions, where dry caves and shelters were utilized as storage, burial, and camp sites, there is an unusually large body of information about basketry, cordage, finger-woven bags, sandals, and wooden implements, including the atlatl but not the bow. Pottery, however, has not yet appeared, though long in use in the Mogollon and Hohokam traditions of adjacent areas. In sum, as a phase that has adopted agriculture without profound modifications in settlement pattern or other branches of technology, which remain substantially on the Archaic level, Basket Maker II is an ideal type specimen of Preformative stage culture as conceived in this paper.

Basket Maker III, or Modified Basket Maker as it is alternatively called, is not so easy to classify (Roberts 1929; Martin 1939; Amsden 1949; Lancaster and others 1954). It is essentially a continuation of Basket Maker II with new features, such as additional varieties of corn, beans, domesticated turkey, pottery, the bow, grooved ax, and maul, none of decisive significance perhaps from a developmental point of view, but the general configuration, particularly toward the end of the period, is rather suggestive of the Formative. There is

now widespread use of substantial slab-lined pit-houses of standardized types, with antechambers, ventilators, and other special features that became embalmed in the small kivas of later periods. There are also in some localities ranges of contiguous one-story surface rooms of crude masonry and *jacal* construction that foreshadow the agglutinated unit type house structures of Pueblo II and III periods. More important perhaps, from our point of view, is the fact that the villages attain considerable size in Basket Maker III, up to 150 rooms or more, and that in some cases large circular structures are associated with them, of presumably religious or ceremonial import, possible prototypes of the Great Kivas of Pueblo III.

In other words, Basket Maker III, in some regions of the Anasazi area, has gone a long way toward the Formative. But the trend does not seem to have continued. In the succeeding Pueblo I period, although there is a progressive development of masonry architecture, there is a tendency for villages to be smaller (Roberts 1931, 1939, 1940). Obviously, without knowing whether these seemingly small settlements were organized into larger communities it is difficult to evaluate this kind of change. At any rate, in Pueblo II (Morris 1939; Brew 1946) the trend toward larger villages is resumed and is accompanied by pronounced architectural improvement, both in construction and planning. All the specialized features of the kiva attained their characteristic form in this period, and the kivas began to be incorporated into the village plan in a manner that indicates foresight and design. At this point we can certainly say that Anasazi culture has reached a Formative stage, and the date is somewhere in the neighborhood of A.D. 900.

Like the Mogollon, the Hohokam of the Gila Basin and Tucson regions of Arizona appears to have grown out of the earlier Cochise tradition, but with the important difference that the later phases of Cochise in these regions have not yet furnished evidences of agriculture. Until such are forthcoming, the Preformative begins with the Vahki phase (Gladwin and others 1937) about 300 B.C. (Wheat 1954) and continues through the remaining three phases of the Pioneer period. Population increase, along with ball courts, canal irrigation, and a presumed step-up of agricultural production in the succeeding Colonial period (Woodward 1931; Haury 1932) mark the beginning of the Formative. That the date, about A.D. 500 (Wheat 1954), is considerably earlier than the shift to Formative in the Mogollon and Anasazi traditions may be due to a greater intensity of diffusion from Mexico. It may also in part be due to the challenge of an unfavorable environment for agriculture, and the response to that challenge in the form of irrigation. The forcing effects of irrigation on culture generally have been frequently pointed out (Steward 1949a).[17]

In discussing the Archaic we have already anticipated much of what can be said at present about the Preformative in eastern North America. Most cultures of the "Middle Woodland" period (Griffin 1952a) would probably qualify as Preformative if we had the necessary information about their economic and social organization, particularly the extent of their dependence

on maize, which for most of them we have to assume without proof. Many of these cultures, however, show levels of development in certain features far above the simple Preformative cultures of the Southwest and Middle and South America. At the same time they show lag in other departments, notably pottery.

Large-scale cultural traditions of the Middle Woodland period have not been worked out. All we can do here is indicate several formulations that have achieved a certain currency in the literature but have not been systematically defined. Most important, perhaps, is the Adena-Hopewell tradition centering in the middle Ohio Valley. The close relationship of the well-known Adena (Greenman 1932; Webb and Snow 1945) and Hopewell (Shetrone 1931) cultures has long been recognized. Archeologists were about ready to accept them as sequent phases of a regional continuity, when the first radiocarbon dates came along and upset everything by appearing to reverse the assumed temporal relationship (Griffin 1951). More recent C14 dates have pretty well restored the situation, and it now appears that the Adena-Hopewell tradition extended from about 800 B.C. to A.D. 1 or later.[18]

The implications of these dates, if correct, are worthy of comment. The climax of Adena-Hopewell is represented by the "big four" Ohio sites: Hopewell (Moorehead 1922a; Shetrone 1926); Mound City (Mills 1922); Seip (Mills 1909; Shetrone and Greenman 1931) in the Scioto Valley in the vicinity of Chilliocothe; and the Turner mounds (Willoughby 1922) on the Little Miami near Cincinnati. This climax development cannot be placed accurately in the time span indicated above but we may assume it was toward the end, say in the last two or three centuries B.C. The technological skill and artistic sensitivity exhibited in some of the objects of stone, obsidian, tortoise shell, bone, copper, silver, and meteoric iron were unapproached anywhere else in North America at this time, and hardly surpassed in Middle and South America. The same may be said of the energy expended in the construction of huge burial mounds, ceremonial earthworks, and fortifications, and here we may include the earlier Adena phase of the tradition as well. The great Grave Creek mound near Wheeling, West Viriginia (Squier and Davis 1848, fig. 56), the Miamisburg mound near Dayton, Ohio (Shetrone 1931, fig. 100), each in the neighborhood of seventy feet high, and the Great Serpent Mound of Adams county, Ohio, though not satisfactorily excavated, are all considered on fairly good evidence to be Adena and may, therefore, date as early as 500 B.C. or earlier. From the standpoint of mass, these monuments will stand comparison with any other New World structures of a comparable date. Granting this high development, both qualitative and quantitative, comparable to that of late Formative cultures in Middle and South America, why do we classify Adena-Hopewell, even in its climactic phase, as Preformative? Simply because we have as yet insufficient evidence of large stable villages, and, although maize has been found at one or two sites, there is no satisfactory indication that it was the dominant element in the economy.

Whether the other well-known "Hopewellian" cultures scattered widely

throughout the East should be regarded as part of the Adena-Hopewell tradition, or as phases of other unformulated traditions linked by what we might call a Hopewellian "horizon," is a complicated question that cannot be adequately dealt with here. Our feeling is that the various Hopewellian phases in Illinois (Cole and Deuel 1937; Deuel 1952), Missouri (Wedel 1938; Chapman 1948), Indiana (Lilly 1937:28ff.), Wisconsin (McKern 1931; Cooper 1933), Michigan (Quimby 1941), and New York (Ritchie 1938) might be drawn into the Adena-Hopewell tradition without stretching it unduly, but so-called Hopewellian cultures farther away, particularly in the south, while possessing individual elements (horizon markers?), show markedly different total configurations. The organization of these cultures into regional traditions is one of the next tasks of Southeastern archeology. A start has been made in the formulation of a Gulf tradition, occupying the Gulf coastal plain from Tampa Bay westward across and up the Mississippi and Red rivers into the East Texas or "Caddo" region (Goggin 1949; Ford 1952; Sears 1954). So far this is little more than a pottery tradition, but, as Sears rightly maintains, where ceramic relationships are so close a general cultural continuity can be reasonably hypothesized. The tradition began on a time level early enough to have incorporated marker elements of the Hopewellian horizon, as in the Santa Rosa–Swift Creek phase of the Florida Gulf coast (Willey 1949) and Marksville and Troyville phases in the lower Mississippi Valley (Ford 1936; Ford and Willey 1940), but its most characteristic expression comes later in the form of the pleasing curvilinear incised and punctated style of Weeden Island (Willey 1949) on the Florida Gulf coast, Coles Creek in the lower Mississippi (Ford 1951), and the Alto Focus in East Texas (Newell and Krieger 1949). Time estimates for this climax period have gyrated wildly between the extreme limits of A.D. 500 and A.D. 1450. The only available radiocarbon date of A.D. 398 ± 175 for corncobs from the Davis site of the Alto Focus (Sample No. C-153, Libby 1952a) is viewed with suspicion by most Easternists.

It appears that in mid-passage the Gulf tradition was impinged upon either by the Mississippi tradition or, more likely, some other as yet undisclosed influence of possible Middle American origin.[19] There seems to have been a shift in ceremonial emphasis, marked by the introduction of rectangular platform or "temple" mounds, as they are designated in Southeastern archeology, but the older conical burial mounds are still in evidence. The recently excavated Kolomoki site on the lower Chattahoochee River in southwestern Georgia (Sears 1951a, 1951b, 1953), with a mixed Weeden Island–Late Swift Creek pottery complex, is an example. There is a parallel situation in Coles Creek and even in some sites of the succeeding Plaquemine phase (Quimby 1951) in the lower Mississippi, in which rectangular and conical mounds are grouped together in the same planned assemblages. The first appearance of rectangular temple mounds has been used to mark the advent of the Mississippi "period" in the Southeast (Griffin 1952a:361). This one-to-one correlation between temple mounds and Mississippi culture has caused a certain amount of confusion in Southeastern studies. We shall have occasion to define Missis-

sippi as a tradition in a later section. Exclusive rights over the temple mound is not one of the criteria. Thus the Gulf tradition is enabled to incorporate the temple mound in some of its phases, and at a fairly early period, without obliging us to say that it has thereby given way to the Mississippi tradition. This, however, makes it rather difficult to bring the Gulf tradition to a close. In the Fort Walton phase on the Florida Gulf coast (Willey 1949) and the more northerly sites of the Plaquemine phase on the lower Mississippi (Cotter 1951, 1952) the culture was beginning to show some of the more symptomatic Mississippi features, but a distinctive Gulf flavor was maintained right through to the end. At approximately this same time we can say that the Gulf tradition had reached a Formative stage. However, even in its later phases, it is noteworthy that the tendency toward a small-scale hamlet type settlement pattern persists. Ceremonial temple mound sites may be large, but they rarely show signs of intense occupation. This is quite different from the large compact town sites of the Mississippi tradition. Until we know a great deal more about settlement patterns in the Southeast and their socio-economic implications, the separation of Preformative and Formative cultures is bound to be a very arbitrary proceeding. About all we can say in summary is that, whereas the developed Mississippi culture in its nuclear area is readily identified as Formative, the late Mississippi-ized phases of the Gulf tradition can be so classified only with reservations.

In the area occupied by the Gulf tradition, but definitely not part of it, is the anomalous Poverty Point culture of the lower Mississippi, which is represented by one of the largest archeological sites in North America (Ford 1954). At the present time it is difficult to relate this phase to any known tradition. It is preceramic, but it is not "Southeastern Archaic." The typological affiliations of the great octagonal earthworks at the Poverty Point site are with Adena-Hopewell, but Poverty Point is stratigraphically earlier than Marksville and the first appearance of Hopewellian "horizon" markers in the lower Mississippi (Ford, Phillips and Haag 1955). The single available radiocarbon date of 399 ± 80 B.C. (Sample No. L-114, Kulp, Feely and Tryon 1951)[20] would place Poverty Point at about the middle of the Adena-Hopewell span. There is as yet no evidence whatever of agriculture in Poverty Point, but the size of the type site and magnitude of the earthworks alone compel us to classify the culture as Preformative.

A number of other Southeastern traditions that lie all or partly within the Preformative might be considered, but we have space to mention only a few of them briefly. The St. Johns tradition of peninsular Florida (Goggin 1949, 1952) seems to have remained on a Preformative level throughout the entire span of its existence. Elements derived from Formative stage cultures appear in its later phases, but there are no signs of accompanying socio-economic changes.

In Georgia and contiguous portions of adjoining states, the so-called South Appalachian or Complicated Stamped tradition—the latter may be the more appropriate name since it consists of one or several closely related pottery

traditions—lies mainly in the Preformative (Wauchope 1950; Sears 1952). Grounded in simple and checked stamped "Early Woodland" phases such as Mossy Oak and Deptford (Fairbanks 1952), which we should probably classify as Archaic, the tradition attained a sudden and unexplained climax, from the qualitative point of view, in the beautiful curvilinear style of the Early Swift Creek phase of south central Georgia (Kelly 1938), generally regarded as on or near the Hopewellian horizon. A parallel development, seemingly deriving directly out of Mossy Oak Simple Stamped, resulted in a rectilinear style called "Early Napier." Both curvilinear and rectilinear styles went through a number of phases and interacted upon each other in a very complicated fashion, their influence spreading far beyond the borders of Georgia, but the associated cultures, so far as we can tell, remained on a Preformative level until the late Etowah and Lamar phases, which will be referred to in a later section.

We have said nothing about the Preformative cultures of the Central Mississippi, Lower Ohio and Tennessee-Cumberland regions. These combine to form what is probably the nuclear area for the formation of the Mississippi tradition, and it will be more convenient to refer to some of the earlier cultures in this area when discussing that tradition in the Formative section.

In the Plains, we have already spoken of the Sterns Creek phase, with squash and beans but no corn, as Preformative. The most extensive and characteristically Preformative culture in this area, however, is the later Upper Republican "aspect" (Strong 1935; Kivett 1949), dating from about A.D. 1200–1500 (Wedel 1947; Roberts 1952). This culture, represented by innumerable small pit-house villages strung out along the first stream terraces from South Dakota across Nebraska, Kansas, and eastern Colorado, seems to represent the initial adjustment of a sedentary farming culture, of presumed eastern derivation, to an environment that offered the additional resource of buffalo hunting. The resulting mixed seasonal economy, and the highly characteristic culture that developed with it, has been called the "Plains Village tradition." Owing to limitations of transport, it remained on a Preformative level throughout the period represented by the Upper Republican and related phases, but, with the advent of the horse in early post-Spanish contact times, villages became a good deal larger and more permanent. The lower Loup phase of east central Nebraska (Dunlevy 1936), for example, thought to be protohistoric Pawnee, could be classified as Formative in terms of the criteria used here, as could the historic Pawnee sites (Wedel 1936). However, if we are correct in assuming that it took the horse to do it, it becomes a slightly spurious Formative.

In the southern Plains, the Antelope Creek phase of the Texas Panhandle (Krieger 1946) shows specific resemblances to Upper Republican and apparently occupied about the same time period. It also is based on a mixed bison-corn economy but differs from Upper Republican in that its marginal relationship is to the Formative cultures of the Southwest rather than the Southeast.

We might also designate as late marginal Preformative the vast number

of post-Hopewellian cultures that have been identified in the Upper Mississippi, Great Lakes, and Middle and North Atlantic areas. This would include most of the "Upper Mississippi" (McKern 1945) and "Transitional and Late Woodland" (Bennett 1952; Quimby 1952; Schmitt 1952; MacNeish 1952) cultures of existing classifications. From the myopic point of view of the present paper, the typological differences between these two groups, however interesting and informative, are outweighed by the fact that both practiced some agriculture, to the extent permitted by the local environment, but depended heavily on hunting and collecting and were therefore not Formative.

Generalizing on the Eastern Preformative data as a whole, we are conscious of a certain amount of taxonomic strain. The fact that many cultures refuse to fit smoothly into a scheme based largely on Southwestern and Latin American norms is in itself a revealing one. It may mean that agriculture's role is not so determinant in this area, particularly in the earlier cultures that we have classified as Preformative. For example, the Adena-Hopewell tradition, which appears to have postdated the beginnings of the Southwestern Preformative by one or two millennia, was apparently in a marginal situation with respect to the spread of agricultural techniques and associated elements. At the same time, it represents the culmination of long periods of development of other techniques, notably polished stone and metalworking, which had already reached an advanced age in the late Archaic. The peculiar physiognomy of Adena-Hopewell is therefore due as much, if not more, to local developments as to relayed influences from Middle America. The same observation does not hold for the Gulf tradition, which seems to be more intimately bound up with Middle America. The fact that, though it had more time, it failed to reach the high levels of Adena-Hopewell is all the more interesting. Poverty Point, on the other hand, seems to be completely original, at least so far as possible influence from the Southwest or Middle America is concerned. As to the South Appalachian tradition, all efforts to derive complicated stamped pottery from the Antilles or Middle America have so far been unsuccessful.

We have referred casually to the Middle American Preformative but have not yet disclosed how very little evidence there is for it. The light thrown by recent discoveries on the early agricultural developments in the Southwest is what we have long expected to find in Middle America. Here, as we shall see, the Formative threshold is at least a millennium earlier, so, all things being equal, we should expect to push back the beginnings of the Preformative to a corresponding extent. However, for reasons that remain extremely puzzling, this expectation has not been realized. Over almost the entire area, cultures antedating the Formative have not yet been found. The only glimpse that we have had of a Preformative stage culture in Middle America is in the extreme northeastern part, about midway between the Southwest and the main centers of Middle American high culture. Here, in the Canyon del Diablo sequence in Tamaulipas (MacNeish 1950), we find a rough parallel to the Cochise-Mogollon sequence of the Southwest. We have already mentioned

the Early Lithic Diablo and Lerma phases and the Archaic Nogales phase. Here our interest centers in the succeeding La Perra phase, a preceramic gathering and food producing culture with abundant plant remains, including a primitive (but not so primitive as Bat Cave) maize. The latter has been dated by radiocarbon measurement at 2494±280 B.C. (Sample No. C-687, Libby 1952a).[21] This is followed by three phases of the Pueblito culture (MacNeish 1947), the first two of which would seem to qualify as Formative, but the third and latest, the Los Angeles, shows a decided falling off and perhaps should be rated as Preformative in the sense of "recessive Formative" as discussed above. Radiocarbon dates for the Pueblito, averaging A.D. 1300 ±150 (Sample No. C-207, Libby 1952b) are probably too late. The earliest Pueblito phase appears to equate roughly with Huasteca II (Ekholm 1944), which has an estimated date of 500 B.C., based on cross-dating with late Formative phases in the Valley of Mexico, which in turn have been dated by Carbon 14. The apparent gap in time between La Perra and Pueblito I, plus the somewhat abrupt appearance of a fully Formative culture with the latter, suggests either that one or more phases are missing in the Tamaulipas sequence or that the complete transition from Preformative to Formative did not take place in this region.

In South America the classic example of a Preformative stage culture is furnished by the evidence at Huaca Prieta in the Chicama Valley of the north coast of Peru (Bird 1948a, 1948b). Here, remains of squash, peppers, gourds, and cotton—but not corn—were found in a preceramic context that would otherwise be classified as Archaic or—if only the stone assemblage of crude flakes and choppers was available—even Early Lithic. These preceramic agricultural deposits had accumulated with little change in artifacts to a depth of forty feet through a period measured by radiocarbon dates of about 2500 to 1250 B.C. (Bird 1951). What the culture may have been like before the discovery, or introduction, of cultivated plants, we do not know. The only earlier culture in the Chicama Valley, which we have already referred to as Early Lithic, has no apparent association with Huaca Prieta. Not at the big mound, but in a nearby midden, Huaca Prieta culture was overlaid by Cupisnique, the phase that marks the advent of the Formative stage on this part of the north Peruvian coast.

Elsewhere in South America a number of cultures might be cited as technically Preformative, but it is doubtful whether any of them represents a truly incipient stage of agricultural development. For example, the Marajoara phase at the mouth of the Amazon (Evans and Meggers 1950) seems to have undergone a regression from previous Formative standards, and the subsequent phases on Marajó and neighboring islands continue in a similar pattern. We have already suggested that such belated agricultural phases could more accurately be designated as marginal or recessive Formative. A large number of other South American cultures could be placed in the same category. In other instances, either the Preformative stage was not reached, as in the east Brazilian and the southern South American areas, or, as in

most of the Amazonian lowlands, the stages antecedent to the Formative are not yet known.

In the West Indies, Formative type cultures come in with the well-developed Cedros and Cuevas phases (Rouse 1953). Stimulus diffusion from these cultures was apparently important in the development of the Meillac phase of Haiti, which might be classified as Preformative in a marginal sense (Rouse 1941, 1953). In Trinidad, Puerto Rico, the Virgin Islands, and Santo Domingo, Archaic cultures were directly superseded by Formative without any apparent stage of transition that might be classified as Preformative.

FORMATIVE STAGE

The New World Formative stage is defined by the presence of maize and/or manioc agriculture and by the successful socioeconomic integration of such an agriculture into well-established sedentary village life. This definition has both developmental and historical connotations. We are dealing with societies of a certain minimal complexity and stability whose population sizes and gross groupings have been made possible by a particular food economy, but we are also treating with a historical phenomenon—the diffusion, or diffusions, of native American agriculture. Potterymaking, weaving, stone carving, and a specialized ceremonial architecture are usually associated with these American Formative societies. These elements are not inevitably linked to American agriculture through any inner causality, and some of them are often found in cultural contexts which are nonagricultural. Seldom, however, are American agricultural societies lacking in all of them. There are insufficient data, as yet, to establish the relative chronological appearances of these trait complexes in the various New World areas, but it is unlikely that they exploded concurrently in one locality to effect a sudden and sweeping cultural revolution. The significance of the American Formative is less one of origins than it is one of function. We have seen that varieties of domesticated maize appeared in northern Mexico and the southwestern United States in the Preformative stage two to three millennia before the Christian Era without producing an immediate and dramatic cultural change. It is most likely that the origins of the Formative will be found to be extremely complex and diverse—the gradual assemblage of elements over considerable expanses of time and area to produce a cumulative and patterned result.

Prehistoric cultures of the Formative stage occupy a geographically central position on the American continents. They are found throughout much of Middle America and most of Andean South America. From these areas they extend northward, with some lacunae, into the southwestern and eastern United States, and in South America they run down the Cordillera to central Chile and eastward along the Amazon and Orinocan drainages of the lowlands. Their chronological span would appear to go back to the second millennium B.C. in Middle America[22] and almost that far in Peru.[23] As noted above, there are early occurrences of maize in New Mexico and Tamaulipas, but there is no indication that agriculture was the primary basis of life in

these cultures or that populations were organized into sizable, stable villages. Even the much later Basket Maker cultures appear to be transitional from Preformative to Formative, and it was not until the second half of the first millennium A.D. that the Pueblo villages appeared. In eastern North America most authorities would agree that the second half of the first millennium A.D. is as early as full-blown agriculture can be placed in this area. The evidence for early agriculture in the east, during the Middle Woodland period, has already been discussed. In lower Central America, Colombia, and Ecuador it is possible that agricultural village life is of an age with that of Middle America and Peru, but there is still little in the way of proof. In the South Andes the Formative may be as late as A.D. 1000.[24] Caribbean and Amazonian archeological sequences suggest considerable time depth for Formative-type cultures in some regions, but, as yet, there are few means by which to correlate these with Mexican or Peruvian chronologies.

In Middle America[25] the earliest known pottery-agricultural phases of several different archeological regions are representative of Formative stage culture. A definition of all of the regional-cultural traditions of Middle America is beyond the scope of this paper, but any one of several such traditional continuities will serve to demonstrate the rise and growth of Middle American civilization on the Formative stage. In the Valley of Mexico and its environs the El Arbolillo I (Vaillant 1935) and the Early Zacatenco (Vaillant 1930) phases are the earliest Formative manifestations. These phases are represented by large village sites of deep and extensive refuse. Handmade figurines, incised or white-on-red decorated ceramics, and metate and mano stones are characteristic artifacts. The Formative stage development of what might be called a Mexican highland tradition can be traced through successive Middle Zacatenco, Tlatilco, Ticoman, and Cuicuilco phases (see Porter 1953). Although there is strong continuity from one phase to the next this should not be misconstrued as a local development alone. Outside influences come into the Valley of Mexico during these Formative stage phases, and one of the most striking of these "foreign" waves is reflected in the Tlatilco pottery and the associated Olmec style figurines. Toward the close of the Formative stage in the Valley of Mexico the trait of platform mound construction appears—Cuicuilco is the outstanding example (Cummings 1933)—as does monumental stone sculpture. Both these traits are then carried through the various phases of the Classic and Postclassic stages in this region.

In turning to the other regional-cultural traditions of Middle America, a number of parallels with the Mexican highland tradition are at once evident. The extent to which these parallels are either historically or developmentally conditioned is one of the major problems of Middle American archeology. The presence of the Olmec figurine style at Tlatilco and in the Early Tres Zapotes phase of Veracruz (Drucker 1943) or the close similarities between Formative stage pottery of Maya lowland Mamom and Chicanel and that of Veracruz and the Huaxteca (Ekholm 1944; MacNeish 1954b) are undeniably the results

of diffusion. But such phenomena as the apparent increase of populations during the Formative cannot be explained by historical forces alone. The idea of the platform or pyramid mound was undoubtedly widely diffused in Middle America during the late Formative, but the conditions for its acceptance from region to region were locally developed.

This suggests the hypothesis that Middle American Formative stage culture, as it developed in the various foci of its regional traditions, proceeded steadily but somewhat unevenly. Most of the great achievements of the subsequent Classic stage are presaged by the Formative, but there is no significant regional clustering of these elements. Ambitious stone sculpture is featured in one region, elaborately decorated ceramics in another, and what appear to be the beginnings of a glyphic system elsewhere (Caso 1947). Certain regions may have evolved particular features because of peculiar natural environmental circumstances or advantages. Other traditional growths, lacking settings with these specific potentialities, may have turned to different courses. Geographical proximities and distances or physiographic barriers undoubtedly influenced the workings of diffusion, allowing an innovation or an acceleration of a trend here, delaying it there. With such cultural growth, which we cannot yet follow and may never be able to follow in all its complexity of detail, the Formative cultures of Middle America gradually assumed what archeologists have come to call a Classic status. Surely, one of the reasons that cultures of this kind could be developed in Middle America derives from the fact of the multiplicity of regional traditional antecedents. The intertwining of the many varied strands of the Formative produced the Classic. Individually, these strands would have been of no greater moment than a culture like the Mississippian of the eastern United States, with its temple mounds, or the Cocle culture of Panama, with its fine pottery and metalcraft. Together, they emerge as Middle American civilization. This does not mean that the Middle American Classic stage was characterized by a single, homogeneous culture or civilization. Regionalism persisted, but it was a regionalism in which the various Classic cultures had assimilated sufficient from each other so that all drew upon a common fund of great depth and richness.

Northwest of Middle America the distinctive patterns which characterize that area become attenuated and eventually disappear. Formative stage culture in the southwestern United States is expressed in the three principal cultural traditions of that area: the Anasazi, Hohokam, and Mogollon. Although Middle American elements are to be found in these traditions, the over-all settings and configurations are distinct entities in their own right, and became so on a Preformative level. The points at which they may be said to have become Formative have already been chosen in the preceding section of this paper. No Southwesternists are likely to agree with any of these choices. We have given priority, in a temporal sense, to Hohokam, because it seems that the evidence for canal irrigation and a presumed step-up in agricultural production in the Colonial period (Gladwin and others 1937; Woodward 1931; Haury 1932), beginning about A.D. 500, are very significant from our present

point of view. We have also emphasized the presence of ball-courts in Colonial period villages. Undoubtedly, the ball-game idea was passed into the Southwest from Middle America, but its adoption and the construction of large community works in connection with it indicate that Hohokam societies were developing sufficient size and co-ordination to exploit such an idea. In the Sedentary period (Gladwin and others 1937) irrigation systems were enlarged and improved; ball-courts were still in use but were reduced in size; villages became compactly planned settlements of adobe houses, in some cases enclosed by walls to form compounds. In the Classic period, beginning about A.D. 1000, the innovations were largely architectural, culminating in the massive, multistoried adobe structures of Casa Grande (Gladwin 1928) and Los Muertos (Haury 1945). To some extent these architectural and settlement changes of the Sedentary and Classic may be attributed to diffusions and migrations from the Anasazi region to the north; yet the continued expansion of the irrigation systems would seem to suggest steadily increasing populations. Thus, although the form or style of the new settlement may have been Anasazi (or Salado), the social and cultural conditions which it reflects were, at least in part, engendered within the Hohokam tradition. The causes of the breakup of Hohokam culture after A.D. 1400 are not fully understood. Soil depletion and irrigation difficulties, internal warfare and external pressures are all cited as possibilities. Whatever the course of events, the prosperous agricultural villages could no longer be maintained, and Hohokam culture slipped back from its Formative stage achievements to a condition reminiscent of a simpler level.

In the case of the Anasazi, our hesitation about deferring the onset of the Formative until Pueblo II times has been clearly acknowledged. It would be possible to make a good case for putting it in the latter part of Basket Maker III, say about A.D. 600, and this would be more in line with our choice in regard to the Hohokam. Neither of the present writers feels competent to pass on such a point. However these two traditions may have compared as to the time of their emergence into Formative status, it does appear that the Anasazi of the great Pueblo III (A.D. 1100–1300) and Pueblo IV (A.D. 1300–1700) periods represents the climax of Formative stage culture in the Southwest as a whole. Athabascan incursions and Spanish conquest had serious effects upon Anasazi culture, and the towns decreased in size and numbers; nevertheless, the tradition remained viable and a Formative type of life persisted and still persists in the modern Pueblos.

The question of Mogollon Formative is rendered difficult by the fact that only in its earlier phases does it present a "pure" and characteristic pattern. After about A.D. 700–800 it was interacting with the Anasazi and Hohokam traditions—both of which appear to have been stronger—in a very complicated fashion. In the Preformative section we referred with intentional vagueness to the close of the long Mogollon Preformative around the time of the Three Circle phase, about A.D. 900. Certainly the succeeding Mimbres phase (Bradfield 1931, 1929; Cosgrove and Cosgrove 1932), beginning about A.D. 1100,

was on a Formative level, but by this time it would be a rash outsider who would say that the culture was still Mogollon.

In sum, the Formative stage cultures of the Southwest, in contrast to Middle America, are seen in their earliest beginnings and show the gradual steps by which marginal agricultural societies may achieve full agricultural status. Perhaps, in an internal sense, Southwestern cultures could be said to have reached a Classic stage in such manifestations as the Pueblo Bonito phase of the Chaco Canyon (Judd 1954; Kidder 1924). But, from an external point of view, the Pueblo Bonito achievement is meager in its over-all architectural, artistic, and intellectual attainments when compared to Classic Middle America or Peru. It has been a commonly held opinion that Southwestern native cultures lagged behind Middle America because the area was geographically remote from the sources of American agriculture and, therefore, started its climb much later. But the early dates for Bat Cave and Tularosa corn in the Southwest force us to re-examine this explanation. We have argued that a varied natural environment and multiple regional-cultural centers in Middle America were significant stimuli for cultural growth in that area. It is likely that the limitations of the Southwest in this regard were the most important deterrents to the rise of a native American Classic stage in that area.

In eastern North America the features we have selected as reflective of an intensive agricultural village life, or a Formative stage, are the rectangular "temple" and "town-house" mounds, the arrangement of these mounds around a central plaza, compact villages of substantial houses with deep and extensive refuse (other than shell middens), and the frequent and abundant finds of maize itself. Such features, along with certain pottery types and other artifacts, are known to be associated, in both space and time, with the Mississippi (formerly "Middle Mississippi") tradition. Most of these elements may be derived from Middle American sources. Although the mechanisms of their diffusion are unknown, they appear as relatively late arrivals in the eastern United States. The Formative in this area is in effect the story of the Mississippi tradition. All Eastern cultures that we are willing to call Formative are either Mississippi or have been strongly influenced by it. It is probably the best-known culture tradition in the East, yet one looks in vain for an adequate definition (cf. Deuel 1935; Phillips 1940). The chief difficulty has been our inability to nucleate the culture geographically. This in turn is partly, if not wholly, the result of the belief—expressed above—that the source of most of its diagnostic elements is Middle America, and that we might therefore expect to find its nucleus in the Gulf coastal, lower Mississippi or East Texas regions. Such expectations have not been fulfilled. We have already shown that certain elements that have been too closely identified with Mississippi, notably rectangular platform mounds, do enter into the Gulf tradition, but, wherever the really diagnostic Mississippi features appear in these regions, they seem to be intrusive from the north. This leads us to the hypothesis that the nuclear area for the Mississippi tradition is somewhere in the central Mississippi (from the

mouth of the Missouri to the Ohio), lower Ohio, and Tennessee-Cumberland regions.[26] It is only in this general area that the tradition seems to have any appreciable time depth, though nothing comparable to the life span of Adena-Hopewell and other Preformative traditions. A few "early Mississippi" phases have been identified, but the stratigraphic evidence is rather tenuous.[27] Such are the Old Village phase at the great Cahokia site in East St. Louis (Kelly 1933; Griffin 1949), the Obion phase in west Tennessee (Kneberg 1952:193), and the Hiwassee Island phase in the Chickamauga Basin of East Tennessee (Lewis and Kneberg 1946). The Macon Plateau phase of central Georgia (Kelly 1938) is an early Mississippi type culture but is clearly an intrusion into a region dominated by the Complicated Stamped tradition (Fairbanks 1952). Following closely upon these scattered early phases, the tradition moves into its climax period in such phases as Monks Mound at Cahokia (Moorehead and others 1929; Titterington 1938), named after the largest rectangular platform mound north of Mexico; Kincaid (Cole and others 1951) and Angel (Lilly 1937:41ff.; Black 1944) on the lower Ohio; Cairo Lowland, in southeast Missouri (Williams 1954); Nodena, or "Walls-Pecan Point," and Parkin, or "St. Francis," in northeastern Arkansas (Phillips, Ford and Griffin 1951; Griffin 1952b); Cumberland, in the Nashville Basin of central Tennessee (Phillips, 1939); Dallas and Mouse Creek in east Tennessee (Lewis and Kneberg 1946); and Moundville, on the Black Warrior River in north central Alabama (Moore 1905, 1907; DeJarnette and Wimberly 1941). Many more phases could be cited, but these are the most intensely Mississippian and seem to define the nuclear area as outlined above. Here is more than sufficient material for a comprehensive definition of Mississippi culture, but it is sufficient for our purposes to note that all these phases, both early and late, are on the Formative level as conceived in this paper.

During the climax period of the Mississippi tradition, an outstanding horizon style, the "Southern Cult," spread throughout the Southeast far beyond the boundaries of true Mississippi culture as understood here (Phillips 1940; Krieger 1945; Waring and Holder 1945). This style, as exemplified in certain designs and motifs on embossed copper plates, engravings upon shell, and on modeled, engraved, and negative painted pottery, along with a number of highly specialized artifact types commonly associated with said designs, is almost certainly of Mississippi origin and serves to measure the extent of Mississippi influence in the late prehistoric period. It poses some very interesting questions in culture dynamics, in that some of the strongest centers for "cult" material are not strictly speaking in the Mississippi tradition. At Etowah, in north Georgia (Moorehead 1932), the cult material seems to be intrusive into a culture that is dominated by the Complicated Stamped tradition of that region, doubtless a thrust from the nearby Tennessee-Cumberland; and Spiro, on the Arkansas River in eastern Oklahoma (Griffin 1952c), which yielded the gaudiest cult material of all, is entirely outside the Mississippi tradition in all other respects, as is Mt. Royal (St. Johns IIb) and other cult "centers" in Florida (Goggin 1952). The horizon style concept

appears to fit the situation admirably but of course is not an explanation.

Getting back to the problem in hand, all the remaining Formative stage cultures in the East can be interpreted on the basis of some sort of contact with the Mississippi tradition. There do not seem to be any other independent Formative traditions in the area. The contact and blending of Mississippi with other traditions, however, produced several examples of what might be termed "emergent" Formative traditions, i.e., traditions that cannot be called either by their old name or Mississippi but were not afforded sufficient time to establish physiognomies of their own. Such, for example, were the mixed Woodland-Mississippi cultures like Fort Ancient[28] in the middle Ohio Valley (Griffin 1943) and Iroquois of New York (MacNeish 1952; Ritchie 1952). In Georgia, as already suggested, Mississippi blended with the Complicated Stamped tradition to form a culture which in the late Etowah and Lamar phases (Fairbanks 1952) in north and central Georgia, and in the Savannah and Irene phases on the coast (Caldwell and McCann 1941), was on its way to becoming one of the strongest and most pervasive Formative traditions in North America. On the Florida Gulf coast Mississippi combined with the Gulf tradition to produce the Fort Walton phase (Willey 1949), which may be tentatively classified as Formative, and in the lower Mississippi River the Mississippi and Gulf traditions blended in the Plaquemine (Cotter 1951, 1952), Lake George and Natchez (Quimby 1942) phases to an extent that makes it difficult to say which predominates. The native culture of East Texas, or the "Caddo" area, tentatively regarded here as part of the Gulf tradition, was strong enough to resist the inroads of Mississippi influence pretty effectively. Even the later phases which make up the Fulton "aspect" (Krieger 1946; Webb 1948) are not very strongly Mississippi-ized. Some of them, moreover, do not seem to be on a Formative level. To the west, on the Plains, we have already spoken of an emergent Plains Village tradition, also partly derived from the Mississippi tradition, which attained in its latest phases a somewhat doubtful Formative status.

In summarizing the Formative stage in eastern North America, several important considerations stand out. The nuclear Formative culture of the area, the Mississippi, has a typical Middle American configuration of temple mounds and plaza assemblages that is entirely lacking in the Southwest. There seems to be little doubt that this basic symbol, and the idea system behind it, were diffused from Middle America, though we cannot say much about the route of that diffusion. It seems to have entered, in a rather small way, into the Gulf tradition some time before the Mississippi climax, but it was not until that climax that it developed to a point which invites comparison with late Formative and early Classic mound structures in Middle America. It is almost impossible not to regard this nexus of traits as closely related to, perhaps dependent on, some new procedures in agriculture hitherto unknown in eastern North America. We can only assume, without proof, that these found their optimum conditions for development, not in the regions nearest their point of entry, but somewhere farther north, in the general vicinity of the confluence of

the Missouri, Illinois, Ohio, and Mississippi rivers, and that the culture brought about by these innovations subsequently spread fanwise from that nuclear area, even back to the south in the direction of the ultimate source of its power. Whereas Southwestern Formative cultures were limited and confined by the small natural localities and regions that were suitable for their growth, the Formative cultures of the Mississippi Valley and Southeast knew no such bounds. The widespread horizon style, or Southern cult, and the attempts at large-scale geographic-political units seen in some of the protohistoric confederacies are phenomena comparable to Middle American developments on the threshold of the Classic stage.

To the south of Middle America the archeological sequences of Honduras and Salvador reveal a number of close relationships with both Mexico and the Maya regions. The Playa de los Muertos, Ulua Bichrome (Strong, Kidder and Paul 1938), and Cerro Zapote (Lothrop 1927) phases are linked closely in their ceramics to the Formative phases of the Peten and Copan. Following these, the Honduran Las Flores and Santa Rita phases (Strong, Kidder and Paul, 1938) feature a Maya-like polychrome pottery, apparently deriving its inspiration from the Tepeu (Smith 1940) phase of the Classic stage of lowland Maya. A still later Naco phase in Honduras has ceramic affiliations with the Postclassic cultures of the Mexican highlands. In spite of these specific historical connections and parallels, it is questionable if the Honduran and Salvadorean developments ever go beyond what we are defining as a Formative stage. Mound building was known in these regions as early as Yarumela III in Honduras—an archeological phase apparently equatable in time with the Ulua Bichrome (Canby 1951)—and mounds, or mounds and ball-courts, were constructed at Tazumal (Boggs 1944), Los Llanitos (Longyear 1944), and Naco (Strong, Kidder and Paul 1938), indicating the persistence or presence of these ideas in Salvador and Honduras in the late pre-Columbian periods. But aside from these architectural features (which in size and elaboration do not compare with the great mound sites of the Middle American Classic) there is little to indicate that Honduras-Salvador ever achieved the full-blown Classic stage. The Middle American contacts in themselves do not seem to have been sufficient to have produced social and cultural phenomena of Classic or Postclassic rank.

Archeological regions and phases have been suggested for Nicaragua and Costa Rica (Strong 1948b), but culture sequences are lacking. In general, mound sites here are of less impressive size or layout than those of Middle America proper. Some mounds appear to have been used as bases for buildings, but, apparently, the superstructures were of perishable materials. Other tumuli may have been built solely for burials. Several types of stone statues occur, frequently in association with the mounds. Ceramics are well developed, and the Nicoya Polychrome style is linked to both Classic Mayan and later Mexican styles by certain decorative motives. Such contacts would date from, perhaps, A.D. 800 to A.D. 1500. Nothing has yet been discovered in Costa Rica or Nicaragua that would relate, specifically, to the Formative stage phases of either Middle America or Salvador-Honduras. Yet in spite of the

fact that the chronological equations between Nicaraguan-Costa Rican archeology and Middle America are with late Classic or Postclassic phases, the archeological remains in this part of lower Central America suggest a level of cultural development no greater than that of the Formative. In some traits, such as pottery and metallurgy, they reveal excellent craftsmanship —undoubtedly attributable to diffusions from both the north and the south. In other aspects, particularly architecture, site size, and site layout, they are not markedly different from southeastern North American Formative cultures.

Archeological regions have been defined for Panama (Lothrop 1948), and, recently, sequence work has begun in one region (Willey and McGimsey 1952; Willey and Stoddard 1954). Large villages and extensive cemeteries are reported. Mounds, apparently for both dwellings and burials, occur (Stirling 1949), but the constructions are very small. There is also some evidence for ceremonial precincts formed by crude stone columns (Lothrop 1937–42), but nonperishable architecture is minimal. There is a fine metallurgical tradition in ornamental gold, tumbaga, and gilded copper which clearly relates to Colombian regions such as the Quimbaya. Except for the Monagrillo culture of the coastal shell heaps to which we have already referred (Willey and McGimsey 1952), all indications are that Panamanian prehistoric cultures such as the Coclé, Veraguas, Chiriqui, and the various ceramic complexes of Darien are relatively late. Level of development, however, appears to correlate with that of the Formative cultures in Middle America and elsewhere.

For the most part, the prehistoric remains of Colombia and Ecuador appear to be upon about the same level as those of lower Central America. That is, everywhere we have evidences, direct or inferred, of sedentary agricultural village life with a competent, or even high, development of pottery and ornamental metalwork. The Quimbaya zone, in the Magdalena River Valley of northwestern Colombia, is an important metallurgical center for the north Andes and for the New World (Bennett 1946a; Banco de La Republica 1948). Both techniques and styles of artifacts probably diffused from this region northward into Panama and other Central American countries. San Agustín, in southern Colombia, is a ceremonial and burial site of considerable size (Preuss 1931), and there are vague relationships between the stone sculptures here and stone statuary in both lower Central America and the north highlands of Peru. However, nowhere in Colombia do we find ceremonial centers with the mammoth architecture of either Middle America or Peru, nor are there evidences of real urban development. The prehistoric Santa Marta or Tairona towns in the northeast (Mason 1931–39; Bennett 1946a) offer the nearest approach to urban centers and to large-scale public works (roads, bridges, etc.); however, the total region is rather small, and the over-all effort falls below the Middle American or Peruvian mark. Although the Chibchans, of the Bogotá Savanna, loom large in some ethnohistoric accounts (see Restrepo 1895) for their politicomilitary prowess, their domain was geographically small and probably not of great size as to population (Kroeber 1946; Haury 1953b).

There is substantial time depth to this pattern of relatively small, seden-

tary agricultural villages in Colombia. The most reliable culture sequences, which are in the northeast and which underlie the later Santa Marta or Tairona cultures, reveal a series of sequent phases (La Loma, El Horno, and Portacelli), all characterized by agricultural communities and polychrome ceramics (Reichel-Dolmatoff 1951). This pattern, essentially Formative in its configuration, persisted throughout Colombian prehistory down to the Spanish conquest and, as far as we can determine, was characteristic of the various regional cultures to which we have referred as well as to others such as the Nariño, Tierradentro, and Upper Cauca (see Bennett 1944a; Ford 1944).

In the Ecuadorean highlands the archeological sequences show a long continuity of Formative-type scattered farm or small village units (Collier 1946; Collier and Murra 1943; Bennett 1946b). In the central and southern highlands there is nothing in the way of large-scale public works or monuments until the period of the Inca invasion.

As an exception to the foregoing, the settlement size and architectural achievements of the Ecuadorean coast may imply Classic and Postclassic stage developments following earlier and clearly Formative cultures like the Pre-Guangala of Guayas (Bushnell 1951). At the site of Cerro Jaboncillo, just back of the Manabí coast, hilltop platform mounds are associated with the remains of stone buildings which contained U-shaped zoomorphic stone chairs, bas-reliefs, and stone statues (Saville 1907–10). This Cerro Jaboncillo culture, probably contemporaneous with the Guangala phase of the Guayas sequence, may be coeval with the middle periods at the large Manabí coastal site of Manta. The Manta middle periods are associated with large, stone-faced, pyramidal mounds. This same Manta site, in its late pre-Columbian or Manteño period, shows extensive dwelling refuse and house foundation remains covering several square kilometers (see Jijón y Caamaño 1930; Sanders ms.). The early ethnographic accounts of the Manabí coast support these late-period archeological data in describing large towns whose wealth and importance was said to have been based upon maritime trade.

To sum up, no prehistoric cultural phase in lower Central America, Colombia, or highland Ecuador is outstanding for a coincidence of great ceremonial construction, monumental art, craft excellence, or urban concentrations of population. For the Ecuadorean coast the data are poorly ordered; yet we know that the Cerro Jaboncillo and Guangala phases and the presumed middle periods at Manta are characterized by specialized temple mounds, monumental stone sculpture, good ceramics, a pottery figurine art of high quality, and a metallurgical industry which produced tools as well as ornaments. The concurrence of all these traits argues for Classic status, as we have defined the stage in this paper. In the later Manteño phase, at the Manta site, there seems to have been a true urban center with extensive dwelling areas surrounding temple structures. Such a site suggests the Postclassic developments of Middle America and Peru. In fact, from the information now available, a Postclassic stage for the Ecuadorean coast is somewhat better documented than a Classic stage. This raises some interesting questions. Did an

Ecuadorean coastal Postclassic civilization rise from the base of a Classic one in that region? Or is it possible that these Ecuadorean cultures moved directly from a Formative stage into the urbanism of the Postclassic as a result of their importance as commercial centers in the trade with the Postclassic kingdoms of the Peruvian coast?

In Peru a number of regional-cultural traditions are first perceived on a Formative stage of development. In each of these regional-cultural continuities certain themes are emphasized, such as the modeled ceramics of the north coast (Kroeber 1926), the multicolored pottery and textiles of the south coast (Gayton and Kroeber 1927; Kroeber 1944), or the subterranean and multistoried stone architecture of the Callejón de Huaylas (Bennett 1944b). Yet this regionalism is interpenetrated by common bonds of technological traits, by developmental changes within these traits, and, also, by the diffusions of specific styles. This kind of unity manifests itself during the early phases of the Formative stage and is still characteristic of native Peruvian cultures at the time of the Spanish conquest (Bennett 1948). In the early Formative phases, such as the Cupisnique of Chicama (Larco Hoyle 1941), the Guañape of Virú (Strong and Evans 1952; Willey 1953), and the Early Supe and Early Ancón of the central coast (Willey and Corbett 1954), villages were small in size and few in number. Maize was known but canal irrigation probably had not commenced. Pottery and small stone carvings were of good quality. The construction of earth and stone or adobe pyramids or other ceremonial buildings seems to have started at this time, as evidenced by the structures in the Nepeña and Casma valleys (Tello 1943; Carrion Cachot 1948; Willey 1951c). An outstanding phenomenon is the widespread Chavín art style, named after the highland site of Chavín de Huantar (Bennett 1944b; Willey 1951c). The late Formative stage phases see the disappearance of Chavín art as an organized style, but they are linked together by a number of technical traits such as the white-on-red vessel painting of Salinar, Puerto Moorin, and Chancay or the widespread "resist" or negative-painting technique on pottery (Willey 1945, 1948). Bennett (Bennett and Bird 1949) has noted that these late Formative phases mark the advent of new food plants, and this also seems to be the time of the first canal irrigation in the coastal valleys and of significant population increases (Willey 1953). Communities remained about the same size as they were earlier, but they increased in numbers. Sizable fortifications were also constructed for the first time.

In drawing the line between Formative and Classic on the north coast of Peru, it is our opinion that it should fall somewhere during the Gallinazo period, which succeeds the Puerto Moorin and precedes the Mochica in the Virú Valley (Strong and Evans 1952). Huge pyramid mounds and a stylistic climax in Gallinazo pottery date from the Gallinazo III phase (Bennett 1950). In the Moche and Chicama valleys, Gallinazo-related cultures may also mark the inception of the great adobe mounds; if not, the succeeding Mochica periods usher in a full-blown Classic stage. On the central coast, big mound building of this kind dates from the Florescent or Classic Maranga 3 phase

(Stumer 1954). On the south coast, Nazca culture has been considered Classic in the light of its magnificent textiles and painted pottery, and there is a likelihood that big adobe mound centers were constructed contemporaneously (Uhle 1924a, 1924b). The nature of Nazca settlement and architecture is, however, poorly known. In the highlands, the Recuay (Bennett 1944b), Huari (Bennett 1953), and Classic Tiahuanaco (Bennett 1934) cultures are considered here as marking the beginnings of the Classic stage.

The prehistoric cultures of northern and central Chile and northwestern Argentina present the Peruvian Formative patterns in a reduced form. Many of the basic food plants, the agricultural techniques, and the domesticated animals (*Auchenia* sp.) are the same as those of the central Andes, but the cultural achievements of the Chilean and Argentinian phases do not equal those of the north. In general, the living zones of the south Andean regions —the desert oases of northern Chile or the *quebradas* and valleys of the Argentine Andes—are small and their agricultural potential limited. In Chile the Atacameñan tradition of the north had some of its roots in the old Archaic stage coastal fishing cultures of the same region (Bird 1943), but with the advent of maize agriculture new environmental adjustments were possible and populations settled in compact oasis villages in the interior. These communities consisted of stone and mud houses, the whole surrounded by a defense wall. Public or ceremonial architecture is unreported (Bennett 1946c). To the south, in the vicinity of Coquimbo, are the settlements of the Diaguita tradition (Lothrop 1946). The environmental conditions in the Chilean Diaguita region are less severe than in Atacameño country, and there is more land suitable for agriculture. Perhaps in consequence of this, houses are more widely scattered. Still farther south, the Araucanian peoples (Cooper 1946) farmed the wooded central Valley of Chile by clearing small, scattered plots. Settlements were small, and there are no central sites with impressive permanent architecture. Political centralization of the Araucanians seems to have been effected only in time of war. As near as can be determined from present data, maize and pottery diffused into Chile from the north or northeast at a relatively late time. There are some indications that this may have been as late as the Peru-Bolivian Derived Tiahuanaco periods, although this is by no means certain (Bird 1943). The plain pottery phases of Pichalo I and II (Bird 1943) and the El Molle (Cornely 1944) culture of the Diaguita region may stem from earlier Peruvian stimuli. Whatever the duration of native farming villages in Chile, this stage of development was not surpassed.

The prehistoric cultures of northwest Argentina are related to those of Chile both in particulars and in general stage of development. Chronological evidence, both direct and inferential, though limited, makes possible some generalizations about the area (Bennett, Bleiler and Sommer 1948). The earliest phases, such as La Candelaria (Rydén 1936) of the central region and Barreales (Debenedetti 1931) of the south, are characterized by small scattered settlements, flimsy architecture, and relatively simple decorated incised and painted pottery types. The middle and late phases of the north, central, and

southern regions see the appearance of compact villages, sometimes fortified or with forts nearby, stone or adobe architecture, small ceremonial structures within towns, the frequent use of bronze and copper, and the predominance of polychrome ceramics. Peruvian influences of the late periods account for traits such as bronze implements and ornaments and, perhaps, for stone and adobe masonry. These late Peruvian Postclassic elements are not found, however, in the same configurations that distinguish their appearance in Peru. Neither northwestern Argentina nor north and central Chile were centers of native American urbanism comparable to that of Peru.

In South America east of the Andes, Formative level culture was established in prehistoric times along the major river systems and much of the Caribbean and Atlantic coasts. We refer here to cultures of the types characterized by Steward (1949b) as "Tropical Forest" and "Circum-Caribbean." Steward has equated a "Circum-Caribbean" level of development with Formative stage cultures in both Middle America and the central Andes. We would also extend the Formative concept to include the lowland forest cultures maintaining stable agricultural villages even though features such as mounds and ceremonial structures are lacking. Such an extension of our definition of Formative embraces, as we have explained, the settled agricultural village life of the southwestern United States and the comparable communities of the south Andes.

In Venezuela there are at least two major cultural traditions through which Formative stage development can be traced. In the Venezuelan Andes and the northwest region (Kidder II 1948a:425ff.) such phases as the Dabajuro and Guadalupe (Rouse 1953, fig. 2) reflect influences probably related to the Rio Rancheria section of northeastern Colombia. Eastward, on the middle and lower Orinoco, there is another traditional center which is characterized by an earlier white-on-red and a later broad-lined incised pottery. The white-on-red pottery idea appears to diffuse out of the Venezuelan Saladero phase into the West Indian phases of Cedros and Cuevas. The later incised pottery is first noted in the Early Barrancas phase of the lower Orinoco; subsequently, it spreads to the West Indies to mark the various late phases of the Taino tradition in those islands. The climax developments of the Formative stage in the West Indies come in these Taino phases, especially in Puerto Rico and the Dominican Republic (Rouse 1953). These developments include the dance plazas, ball-courts, and specialized sculptured stone paraphernalia associated with them.[29] The Igneri, or Pre-Taino, manifestations, while reflective of an agricultural village life, do not possess these more elaborate ceremonial features.

Formative stage cultures appear at the mouth of the Amazon River with the Marajoará phase. Evans and Meggers (1950) have pointed out that the Marajoará culture, with its deep village refuse deposits, special cemetery mounds, and elaborately painted and modeled pottery (Palmatary 1950), breaks with the earlier traditions of the delta. We have already noted, however, that the Marajoará culture disappeared from the region and was replaced

by other cultures of a recessive Formative type. Evans and Meggers see in this the failure of intensive agricuiture on the flooded Marajó Island flatlands and the departure of the people who brought this new subsistence pattern to the delta (Meggers 1954).

Elsewhere on the Amazon, at Santarém (Palmatary 1939), Manaos (Howard 1947), and near Trinidad (Nordenskiold 1913) on the Mamoré, there are evidences of settled villagers manufacturing competent pottery and, presumably, practicing agriculture. On the Atlantic coast the Guaraní tradition may have been the first diffusion of an agricultural way of life southward, eventually reaching the Paraná delta (Lothrop 1932).

CLASSIC STAGE

In our analytical discussions of the Formative stage we have previewed the Classic. Curiously, the Classic stage of New World prehistory, although easy to identify, is most difficult to define. This would seem to be related to the circumstances that the essential Classic criteria are qualitative and relative rather than quantitative and absolute. In the previous stages we have been dealing with more or less objectively verifiable phenomena: stone technologies, domesticated plants, or the sizes of prehistoric communities. Subsequently, with the Postclassic, we will argue that urbanism, secularism, and militarism provide a basis for the definition of that stage. But for the Classic the criteria are those more subjectively perceived characteristics of aesthetic excellence, religious climax, and general florescence. They take on meaning only in the context of what has gone before and, to some extent, what comes afterward.

In establishing standards for Classic stage achievement our frame of reference has been the hemisphere. As stated in the foregoing discussions, we appreciate the historical semi-independence of the various New World cultural traditions and concede that a "classic" stage could be defined within the limited context of any of these traditions. Such a formulation would have meaning for the understanding of culture growth for a particular area and tradition; yet in an evaluation of hemispheric scope—one which considers all of agricultural America as a kind of vast historic entity—only two peaks are outstanding. From this point of view the Classic stage in the New World is limited to Middle America and the central Andes.

The American Classic stage is characterized by superlative performance in many lines of cultural endeavor. It sees not only the mastery but the conjunction of technologies and arts in single cultures and societies. The various and scattered inventions and innovations of the Formative are now drawn together into rich, diverse, and yet unified patterns. The Classic is the stage of great artistic achievements in so far as greatness can be appraised not only subjectively but by the evident time, care, and emotion that have been devoted to the artistic products. It is the stage of monumental and ambitious architecture, and this architecture, in the form of pyramids and special buildings, seems to have been dedicated primarily to religious purposes. Fine specialized craft products for burial furniture, ceremonial uses, or as luxury

items were turned out in profusion. In the Classic cultures of both Middle America and Peru there is evidence of strong social class distinctions and of heavy pomp and dignity surrounding the ruling classes. Intellectual interests as well as the arts flourished in Middle America with the perfection of writing and astronomy. In Middle America, and to a lesser extent in Peru, there was active trade between the Classic regional centers in ceremonial and luxury goods. In spite of this trade it is noteworthy that a strong regional ethnocentrism is reflected in sharply differing art and architectural styles.

The Classic civilizations of Middle America center in the Valley of Mexico and its environs, Oaxaca, the Guatemalan highlands, the Peten-Usumacinta-Motagua lowlands, and coastal Veracruz. Other foci, of somewhat less brilliance, are the Panuco River, on the Gulf of Mexico, and Guerrero and Michoacan in western Mexico. The status of regions such as Jalisco, Colima, and northwestern Mexico is less clear. All these last lie within the historical orbit of an over-all Middle American tradition, but their native cultures appear considerably below Classic standards in the arts and architecture. The chronological span of the Middle American Classic civilizations probably varies regionally, but, in approximate terms, these developments seem to have begun around the beginning of the Christian Era. Gauging chronology by early dated Maya monuments (with the 11.16.0.0.0. correlation), the round figure of A.D. 300 is frequently given as a starting point for the Maya Classic of the Peten.[30] Radiocarbon dates on late Formative phases in the Valley of Mexico would indicate that the Teotihuacan culture of that region had its Classic inception about contemporaneously or only a little earlier. Cross-datings with the Monte Alban sequence in Oaxaca (Caso 1938) and the Kaminaljuyu sequence of the Guatemalan highlands (Kidder, Jennings and Shook 1946) suggest that the apogee of the Zapotecan and highland Maya traditions is roughly coeval with the central Mexican highland and Maya lowland Classic cultures. Tajin Totonac (Garcia Payon 1943) of central Veracruz is, perhaps, only slightly later. The La Venta-Middle Tres Zapotes phase (Drucker 1952) of the Olmec regional-cultural tradition of southern Veracruz and Tabasco may, possibly, be somewhat earlier than other early classic developments; but, if so, it quite likely overlaps with them chronologically.[31] The terminal dates of the Middle Classic cultures may coincide rather closely. The figure of A.D. 900 (11.16.0.0.0. correlation) is put forward as one ending for the Maya lowland Classic, A.D. 650 (12.9.0.0.0. correlation) as another. The apparently abbreviated and not well-defined Teotihuacan IV (Tlamimilolpa) phase of the Mexican highland Classic may have closed prior to the collapse of the great lowland Maya ceremonial centers, or it may have run contemporaneously with them (see Armillas 1950). A reasonable guess-estimate for the fall of Teotihuacan is about A.D. 800. According to the best archeological cross-referencing that can be effected, the end of the other Middle American Classic cultures is co-ordinate with these. In brief, and in gross, the Classic phases span the first millennium A.D., or most of it.

The content of the Middle American Classic civilizations is well-known

and needs no detailed itemization in this survey account (see Thompson 1954; Brainerd 1954; Linné 1934; Caso 1938; Caso and Bernal 1952). The Classic culture of the Maya lowlands has its inception with the occurrence of the Maya corbeled vault, the initial series dates and stelae, and the ornate and unique Maya art style as this is expressed both in sculpture and painted pottery (Spinden 1913). The first appearance of this complex is in the Peten, and there is every reason to believe that it evolved, *sui generis*, in this locality or in nearby regions. During the earlier part of the Maya Classic, sometimes designated as the Tzakol phase (Thompson 1943, 1945; Smith 1940; Proskouriakoff 1950; Morley 1946), ceremonial centers with stelae and characteristic art were first constructed in the central Peten (Uaxactun, Tikal). From here the Classic features spread to Oxkintok (Thompson 1945), in Yucatan, and southeastward to Copan in Honduras. Somewhat later the great sites of Yaxchilan and Piedras Negras (Maler 1901–1903; Satterthwaite 1933) were established on the Usumacinta drainage to the west. Certain ceramic cross-datings, utilizing the basal-flanged bowl form of Tzakol and the frescoed tripod jar of Teotihuacan II-III, indicate that during this earlier half of the Maya Classic the other major civilizations of Middle America were becoming firmly established. It was at this time that the principal monuments of Teotihuacan—the pyramids of the Sun and the Moon and the Ciudadela—were constructed and the distinctive pottery and moldmade figurine styles of that culture came into being (Vaillant 1941; Armillas 1950). Similarly, the Monte Alban IIIa phase of Oaxaca and the Esperanza phase at Kaminaljuyu were, respectively, the first real blossomings of Classic type cultures in those regions.

Aside from the few rather specific cross-finds of pottery and occasional items of architectural detail, Tzakol, Teotihuacan II–III, Monte Alban IIIa, and Esperanza are alike only in that each expresses this first full vigor and brilliance of a regional-cultural tradition. As we have stated, the art styles are all unlike. Certain gods, or god-themes, seem to have been held in common by some of these cultures, but the particular expressions are quite different. Monte Alban shared the trait of writing with the lowland Maya, but the glyphic system is distinct and less developed. Competence in sculptural art was a feat of the Maya but less so for the other Classic phases except the Olmec La Venta. The trick of the ceramic mold possessed by Teotihuacan was not shared by other Classic cultures until later. From these facts it is evident that two fundamental forces were at work in these Classic cultures. Intercommunication existed among them and was an important factor in their growth. They profited from being a part of a larger community of ideas than did the various cultures of the Middle American Formative. Yet this intercommunication and interchange were by no means all-embracing. Technologies, elements, things—these were exchanged; but complete idea systems remained regionalized. How this stylistic regionalism may be interpreted in terms of sociopolitical structure is a major problem for Middle American prehistorians.

In the Maya late Classic of the lowlands (the Tepeu ceramic phase) (Smith 1940; Proskouriakoff 1950; Morley 1946) the number of active ceremonial centers increased greatly. Huge building programs were undertaken. As in the early Classic (and the late Formative) constructions were flat-topped pyramids and platforms grouped around rectangular courtyards or plazas. Temples and palaces were elaborately carved and decorated with sculptures. In the late Classic the palace-type building—generally containing more rooms and situated upon a lower platform than a temple—became somewhat more common than in earlier times. The function of these Maya centers seems to have been largely religious and ceremonial. These were the integrating points in the network of Maya culture. It was in these centers that the peasantry of an agricultural society gathered to be instructed and inspired by its priest-leaders. Regulators of agriculture, guardians of the seasons, and of time, the Maya aristocracy was maintained by, and in turn maintained Maya civilization with, a remarkable pact of mutual faith rather than by force. When this pact dissolved, so also did the structure of Maya Classic society.

The decline and abandonment of the Maya Classic centers at about A.D. 900 (11.16.0.0.0. correlation) probably was preceded by the decline of the other regional Classic cultures. Teotihuacan appears to have been destroyed in the second half of the first millennium A.D.—quite possibly by invaders identified with the Tula-Toltec culture (Armillas 1950). Bearers of this same Tula-Toltec culture moved into other parts of Middle America at this time or shortly thereafter (Tozzer ms.). The breakdown of the old regional states or confederacies of the Classic stage may, in part, be attributed to these invaders or to waves of social and political disruption and dislocation which they set in motion on the northern frontiers of the high civilizations. Other causes for these sweeping and radical changes in the cultures of Middle America at the close of the Classic have also been advanced. One of these is overpopulation or the pressures of steadily increasing population. Archeologists have no sure evidence here. In the Guatemalan highlands there are indications that the population around Kaminaljuyu was as great, or greater, at the close of the Formative than at any time thereafter (Shook and Proskouriakoff ms.). In the Valley of Mexico, at Teotihuacan, there is little doubt that population was more densely massed around that important center in the Teotihuacan IV phase than in the preceding II and III phases (Armillas 1950; see also Linné 1934); but whether this tendency toward urbanization was an expression of absolute increase for the Valley of Mexico or whether it was a matter of rearrangement and grouping of peoples already there is a problem for future study. In the Maya lowlands we know that more ceremonial centers were constructed in the latter part of the Classic than in the earlier centuries. This certainly suggests an over-all population increase for the jungle country, but lack of adequate settlement pattern information in Maya archeology leaves the question open.

The Peruvian Classic cultures, to which we have referred in our discussion of the Formative, include the late Gallinazo-Mochica phases of the north

coast, the Maranga 3 (Stumer 1954) of the central coast, Nazca of the south coast (Gayton and Kroeber 1927), Recuay (Bennett 1944b) and Cajamarca II and III of the north highlands (Reichlen and Reichlen 1949), and Classic Tiahuanaco (Bennett 1934) and, probably, Pucara (Kidder II 1948b, ms.) of the south highlands. Dating control is poorer here than in Middle America. Recent guess-estimates (Strong and Evans 1952; Willey 1953), while lengthening the Formative stage phases back in time, have still held Classic cultures, such as late Gallinazo and Mochica, to the last half of the first millennium A.D. Radiocarbon dates, however, tend to push the beginnings of the Classic phases back to the opening of the Christian Era or even earlier.[22] Terminal dates for the Peruvian Classic are guesswork plus historical reckoning (Rowe 1945). There has been a general and provisional acceptance of A.D. 1000 as a closing date. If Classic beginnings are lowered on the chronological scale, perhaps this figure should also come down. The best we can conclude is that Peruvian Classic cultures flourished, as did those of Middle America, during the first millennium A.D. and that they were more or less contemporaneous with each other.

The regionalistic tendencies of the Peruvian Formative are crystallized on the Classic stage into distinctive civilizations and styles. On the north coast the early trends for public building are brought to fulfilment in the massive flat-topped adobe pyramids and palace complexes of the Chicama, Moche, Virú, and Santa valleys (Willey 1953). At the same time the old north coast predisposition for three-dimensional or modeled art receives full scope in Mochica ceramics. In this same region metallurgy, which also had beginnings as early as the Chavín horizon (Lothrop 1941), is further developed to include casting, alloying, annealing, soldering in gold and copper, and gilding, as well as the manufacture of copper weapons and helmets (Root 1949a; Larco Hoyle 1938–39). The south coastal regional tradition produced a contemporaneous but separate brilliance. Multicolor painting of pottery, which began with the Formative Paracas phase (Kroeber 1944), reached a peak in the Nazca ceramics; and the emphasis on gorgeous textiles, another Paracas trait, was also maintained in Nazca. Metallurgy, on the other hand, remained in its infancy in this south coastal section, at least until very late Nazca times (Root 1949b; Lothrop 1951).

Throughout the highlands there is a strong tradition of stone architecture. The temple at Pucara is a Classic stage example with its dressed stone blocks, complex over-all plan, numerous compartments, and subterranean chambers (Kidder 1948b; Bennett and Bird 1949). The famous Calasasaya enclosure, the monolithic gateway, and the great stairway at Tiahuanaco are even more notable examples (Posnansky 1945), and the carved stone statues of both Pucara and Tiahuanaco are the outstanding representations of Classic art in the south highlands. Farther north, the stone carving and multiple-storied stone buildings of Recuay show the continuity of an old regional tradition (Bennett 1944b). In this instance, the earlier stone masonry and sculpture at

Chavín de Huantar appears superior to the Recuay developments. As stylistic affinities indicate that Chavín de Huantar is approximately contemporaneous with the Formative coastal Chavín cultures, we may have here an example of unconformity between time horizon and stage—at least in so far as architecture and sculpture are concerned.

Concerning settlement and community size, it is of interest to note that large population clusters first came into being during the north coast Classic (Bennett 1950; Willey 1953). And these clusters, as was the case with Teotihuacan in central Mexico, are formed around pyramid or temple centers. An excellent example of this is the Late Gallinazo period aggregation of 30,000 adobe-walled rooms around the Gallinazo pyramid in the Virú Valley. Settlement surveys have not yet been made in many parts of Peru, but such studies as are available tend to confirm this urbanization tendency, at least for the north coast of Peru (Schaedel 1951). Another Classic change in north coast Peruvian sequences is the appearance of buildings composed of large rooms, courtyards, and corridors. These are usually in conjunction with, or nearby, the great pyramids (Willey 1953:356). They have been interpreted as "palaces" or special public or governmental buildings. The comparability of this trend with a similar one in Middle America has been pointed out (Adams ms.).

There are many differences between the Peruvian and the Middle American civilizations which we have designated as Classic, and these differences are configurational as well as in content. Yet, in spite of these, there are some similarities in over-all configuration, particularly with reference to the place and apparent significance of the Classic stage cultures in each historical setting. In the Guatemalan-Mexican regions, as well as in Peru, the Classic cultures take form out of a somewhat less differentiated Formative base. That is, in each major area there is a greater over-all homogeneity of culture in the Formative than in the Classic. It is also of interest, from the point of view of history and diffusion, that this homogeneity is shared between these two areas. Subsequent to the Formative, differentiation rises in accordance with regional interests. Many of these Classic interests or preoccup tions can be seen in certain tendencies manifested in the Formative, but they .re underlined and dramatized during the Classic stage. Another phenomenon is also noted at this point. At the same time that regional differentiation moves to its Classic completeness in the various geographical localities of Peru and Middle America, there is a growing trend for each of these two great culture areas, or co-traditions, to diverge from one another. Thus, despite the parallelisms which we see, it is also a fact that the civilizations of Middle America and Peru are more unlike each other, both in content and pattern, during their Classic stages than at any time previously or later. In the Formative they present a cultural similarity and evenness due, presumably, to the common possession of many historically interrelated traits of a New World sedentary-agricultural way of life. In the Postclassic there is, again, a leveling

of a more complex sort, historically distinct in the two areas and deriving, as near as we can tell, from internal social and political causes. The Classic cultures, between these two, enjoy the greatest freedom and independence from the linkages of either historical or functional causality.

POSTCLASSIC STAGE

As we have intimated, the Postclassic stage in the New World is defined by the features of, or tendencies toward, urbanism, secularism, and militarism. We are dealing here with phenomena which are concrete or which leave concrete evidences in the archeological record. It must be noted, however, that relative or comparative values also enter into this definition. The urbanism of the Postclassic does not deny some similar trends in previous stages, nor do the militaristic and secularistic aspects of this new stage stand in complete contrast to pacific and sacred modes of prior social and cultural behavior. There are, nevertheless, indications of shifts in emphasis in many of the cultural traditions of Middle America and Peru in this Postclassic stage so that city growth, warfare, and nonreligious leadership reached an importance not heretofore accorded them. Another characteristic of the Postclassic is that it was a stage of widespread movements of peoples and idea systems throughout each of the two major areas.[38] Diffusion between regions in the Classic in both the central Andes and Middle America was either the not easily detectable movement of technological ideas and isolated elements or the trade in actual objects and manufactures. On the Postclassic we see the wide, interregional transferences of total art and architectural styles. The mechanisms behind diffusion are debatable, but taken in the general context of either Middle America or Peru at A.D. 1000 it is reasonable to interpret them as actual movements of large groups of people often accompanied by military force. There are, too, certain qualitative criteria that help distinguish the Postclassic. Whereas the Classic was a stage of aesthetic florescence, there is a waning of this in the Postclassic. In several localities it is difficult to characterize it as other than an era of artistic decline. In some regions of both Middle America and Peru, this tendency is seen in standardization and mass production of objects.

The Postclassic centers of Middle America are those well known in the literature of the ethnohistoric period. Aztec Tenochtitlan in the Valley of Mexico is the most famous example (Prescott 1843; Vaillant 1941). Mitla in Oaxaca (Parsons 1936), Tzintzuntzan in the Tarascan country, Totonacan Cempoala in Veracruz, and Mayapan (Roys 1950) in Yucatan are others. Most of these cities were seats of power in the late centuries just antedating, or coming up to, the Spanish conquest. Somewhat earlier, falling in the centuries between A.D. 800 and A.D. 1200, was another set of centers such as Tula, in Hidalgo (Acosta 1940, 1944, 1945), and Mexicanized Chichen Itza (Morris, Charlot and Morris 1931; Tozzer ms.). The total time range of the Middle American Postclassic varies, regionally, as did the Classic. In the cen-

tral Mexican highlands its beginnings may be placed as coincident with the fall of Teotihuacan IV culture and the concomitant rise of Tula (*circa* A.D. 800). Elsewhere, it may be a century or two later. The terminus of the Postclassic is marked by the early sixteenth-century arrival of the Spanish, who found Tenochtitlan full of vigor and Mayapan abandoned.

Peruvian Postclassic cultures include all those of the Tiahuanaco horizon and of later times. Their time span is approximated as of about A.D. 1000 to A.D. 1532 (Rowe 1945). In final years Inca sovereignty spread over all Peru and into adjacent Andean territories (Means 1931; Rowe 1946), but just prior to this there were important regional kingdoms such as the Chimu (Rowe 1948), Cuismancu, and Chincha (Rowe 1946) of the coast and numerous smaller states in the highlands.

The trait of urbanism, with its implications for the social, economic, and political life of the peoples and cultures involved, appears to be one of the most significant in our consideration of an American Postclassic stage. The minimal definition of urbanism which we will offer here is a purely formal one. It takes as its basis large numbers of peoples (numbering into the tens of thousands) living in compact, continuous arrangement in a stable, sedentary manner. There are a number of corollaries which may be read into this minimal definition if we wish to go from formal description to functional interpretation. For example, on the native American scene it follows that a substantial portion of an urban population cannot be direct food producers—at least not with the available aboriginal means of land transport. Thus, there must be outlying and supporting populations for the city proper. Within the city substantial numbers of the nonfood-producers cannot be chiefs, priests, and their entourages, but some must, of necessity, be craftsmen and handlers of produce. These may be either government employees, as in the Inca system, or independent artisans and merchants, as with the Aztec. Finally, the American data show that the cities all contain within themselves certain political and religious functions. In fact, these functions, and their material counterparts—the pyramids, temples, and palaces—seem to have been the nuclei around which the urban zone originally developed. It is quite possible to conceive of a city which did not develop in this fashion, or which did not incorporate politico-religious functions of paramount importance, but this does not appear to have been the history or nature of the native American urban centers which we are considering.[34] In brief, the native city of the New World Postclassic had large population aggregates, was the economic (and probably social, political, and religious) hub for outlying populations, maintained complex and diverse divisions of labor among its citizens, and was a seat of politico-religious power.

We know that in ancient Peru cities so defined came into being on the Classic stage, at least in some regions such as the north coast. There is an important difference, however, between the Peruvian Postclassic city and its presumed Classic stage prototype. The Classic population aggregate appears

to have grown by accretion, somewhat haphazardly, around a temple center, while the Postclassic city follows a planned or partially planned layout (Willey 1953:396–99). It is as though the necessity for, and idea of, accommodating great numbers of inhabitants in a relatively small space had been accepted in Postclassic times whereas on the earlier stage the problem had not yet been dealt with consciously. We are not certain of the time of the first appearance of the "planned city" in Peru, but there are several indications that it may have marked the beginnings of the Postclassic on the Tiahuanaco style horizon (Willey 1953:412ff.; Stumer 1954). By the latter part of the Postclassic the symmetrically arranged urban center was characteristic of the north coast, e.g., sites such as Chan Chan, Pacatnamu, El Purgatorio (Schaedel 1951).

Coexistent with the Peruvian cities of the Postclassic are the aspects of secularism and militarism which we have used in our definition of the stage. The big religious centers and shrines dominated prehistoric Peru during the Formative and, particularly, the Classic. Beginning with the Castillo at Chavín de Huantar and the other Chavín horizon temples in the Nepeña and Casma valleys (Tello 1943), the temple pyramid reached gigantic proportions in Classic stage constructions such as the Huaca Cortada in the Chicama valleys, the Pyramid of the Sun in the Moche Valley (Kroeber 1925), the Gallinazo and Huancaco mounds in the Virú Valley (Bennett 1950; Willey 1953), and the Maranga pyramids in the Rimac Valley (Jijón y Caamaño 1949). In the Postclassic, pyramids continued to be built, but these were usually placed within the enclosures of large architectural complexes (Schaedel 1951; Willey 1953; Stumer 1954). Like the pyramids within the walls of Chan Chan, they tended to be smaller in absolute size than those of the Classic, and they lose still more in impressiveness by the size and numbers of the palaces and city buildings which surround them. This is, admittedly, inferential evidence for the decline of religious prestige and a complementary rise in secular power, but there are other trends which support these inferences drawn from architecture. During the Postclassic, Peruvian art, as a whole, moves away from representational or what could be interpreted as god themes. Following the Tiahuanaco horizon the drift is toward stultification and geometric decorative art. Also, we know from ethnohistoric sources that the Inca state was highly secularized. The Inca sun-god was imposed upon conquered peoples, but there was no concerted effort made to obliterate local religions (Means 1931; Rowe 1946). Power was conceived of as military and bureaucratic might.

Militarism seems to have been a force in old Peruvian society from an early time. In this, as in many other of its trends and emphases, Peru is distinct from Middle America. Large fortifications of a refuge nature have been identified as far back as the late Formative White-on-red horizon (Willey 1953:358–59), and fortified strongholds, or "castillos," are a common feature of north coast Classic. There seems little doubt, however, that organized warfare was stepped up at the close of the Classic and throughout the Post-

classic. The Tiahuanaco stylistic horizon was probably propelled by military force (Willey 1948), and long-distance roads date from this time (Willey 1953:370). Essentially, the difference between Classic and Postclassic warfare in Peru was probably one of scope. Fighting was, undoubtedly, intense on an intravalley or small intervalley scale in the Classic, but wide-scale geographical strategy and maneuvers belong to the later stage. This is indicated by a change in the nature of fortifications as well as by ethnohistoric accounts of Inca activities.

The history of Middle American urbanism cannot be followed in the same detail as that of north Peru, but, as we have remarked, the Teotihuacan IV phase in the Valley of Mexico was a foreshadowing of the city life seen later in sixteenth-century Tenochtitlan. The Aztec city, estimated at 60,000 persons, was supported by a thick fringe of chinampa (floating garden) farmers on its margins and around the shores of Lake Texcoco. A mobility factor—water transportation—was almost certainly of importance in the maintenance of a community of such size, and this same factor may well have been important in Teotihuacan times. Engulfed within the city were temple pyramids, in much the same fashion as in Postclassic Peru. There is not much question that religion was always a stronger force in the life of prehistoric Middle America than in Peru, but there are indications that secular and military powers were in the ascendancy during the Aztec regime. The Tenochcas rose to dominance in central Mexico during several centuries of bitter intercity fighting following the collapse of Teotihuacan, and in this period the war-god rose to a commanding position in Aztec society (Vaillant 1941).

The increasing importance of militarism, secularism, and urbanism in Postclassic[35] Mexico and Central America has been discussed at length by others (Armillas 1951); but, apparently, these trends do not march evenly in all quarters of the Middle American area. There are some indications that the highland basin environments were more propitious for urban growth than the lowlands (Sanders 1953); thus, the urban classification seems to fit better in west Mexico, Oaxaca, and the Guatemalan highlands than in the east coast or Maya lowlands. Yet there are significant variations among the highland regions. In the Guatemalan highlands some of the largest site zones date from the Formative. The Miraflores phase at Kaminaljuyu offers evidence of a great occupation zone and politicoreligious center outstripping in size anything that came later (Shook and Proskouriakoff ms.). Guatemalan highland late Postclassic sites are well fortified, however, and attest to a period of widespread warfare and strife linking up with the disruptions in central Mexico. In the Maya lowlands at Chichen Itza the Tula-Toltec art and the new constructions at an old Classic ceremonial center suggest that long-distance conquest was a feature of the early Postclassic in that region. Just how much of an urban center Chichen Itza ever became is not clear. There are numerous house mounds around the center, but it is unlikely that the dwelling pattern here was comparable to that of compact Tenochtitlan. Later, however, the Yucatecan Maya attempted an urban center at the walled site of Mayapan.

Within the enclosing wall some 4,000 household units have been mapped over an area about 3 by 2 kilometers in extent (Ruppert and Smith 1952). It is the most urbanized of any known Maya site, but whether the greater part of its inhabitants lived within the walls most of the year following nonfarming occupations is still a matter for conjecture. In any case, we can conclude that urbanization was decidedly less successful in the Maya lowlands than in the Valley of Mexico.

There is an important phenomenon connected with the Postclassic stage of development that has not been significant heretofore. This is, in effect, the large-scale exportation and implantation of these Postclassic features of militarism, secularism, and urbanism into and upon other cultural traditions where they had been lacking. Mayapan may be such an example, deriving its Postclassic patterns from central Mexico. In Peru this is certainly the case with the Inca empire. Roads, fortifications, and administrative quarters were constructed by the Inca from Ecuador to central Chile. These traits and much of the Inca system must have had profound effects upon the local inhabitants, although we do not know to what extent these conquered cultures were modified. It is possible, too, that Postclassic phenomena exerted equally great, but less direct, influences in ways other than conquest and incorporation. It has been suggested here that real cities may have grown up on the Ecuadorean coast in response to the rich trade with the Postclassic Chimu kingdom of the north Peruvian coast. If such is true, it would be a recapitulation of the process which gave rise to some of the ancient Mediterranean trading cities of the Old World. It is an excellent example of the interlocking effects of history and internal culture development.

EVALUATION

Before attempting an evaluation of the classification offered in these pages, let us make it very clear that we are under no illusion that it is anything remotely resembling a "natural" evolutionary system. There is nothing inevitable about six stages; they might as well be four, or even eight. Nor is there any "law" that says that all New World cultures must pass through these stages one after the other in their proper order. We cannot even say, for example, that a Preformative stage culture will in the "natural course of events" pass on to a Formative stage. Whether or not it will do so depends on a multiplicity of factors, not all of which are in the domain of culture. As we have previously stated, culture is not an independent order of phenomena intelligible in terms of itself alone (the "cultural superorganic"), and from this it follows that there are no irreversible processes of cultural development. We are not attempting to impose an evolutionary determinism on the data of New World archeology. If such an impression has been given, we have either failed to understand or to communicate what we are trying to do.

Nor can we accept the criticism that ours are not the "right" stages, if such criticism carries the implication that right stages are to be found, given

the necessary information. This attitude involves an important methodological distinction between what we have called culture-historical integration and developmental interpretation, our present concern. The aim of the first is the organization of archeological data in terms of a real world. However far short of attainment it falls, the object is the formulation of archeological units that have (or rather had) a correspondent historical reality. It therefore seems possible that such units may be judged as to their "rightness." In developmental interpretation, on the other hand, we abstract, not only from the primary data but from the above said integrated units as well, certain characteristics that seem to have significance from the point of view of the general development of New World culture. In an over-all sense the sequence is historical as well as developmental, but the individual segments can have no correspondent historical unity or reality, consequently the touchstone of "rightness" is irrelevant. This disposes of the idea that large-scale developmental formulations must not be "set" until we know all about them—which will be never.

We have, therefore, not hesitated to formulate six developmental stages for New World archeology although we are without any illusions about their provisional, not to say ephemeral, nature. The kind of criticism we welcome would revolve around questions such as the following: (1) Does the scheme work? (2) Does it furnish any new insights on general or specific New World problems? (3) Does it facilitate comparisons between Old and New World archeology and the incorporation of the latter into the corpus of universal prehistory?

The all-important question is the first. Does it work? Not too well, it must be confessed. It is impossible to conceal the arbitrary and subjective character of many of the sample determinations in the preceding pages. There seem to be three primary sources of difficulty: (1) that the scheme attempts to be developmental and historical at the same time; (2) that the criteria are largely technological or typological in the earlier stages and socioeconomic or configurational in the later; and (3) that the scheme does not allow for or emphasize cultural regression in the instances where it occurs. That the first difficulty has produced a good deal of ambiguity is freely acknowledged. It is not always clear, even to the writers, whether a given culture is assigned to a certain stage on inherent developmental grounds or for reasons that are in effect historical interpretations. The excuse is that we seldom have enough of the kind of information required to make strictly developmental determinations. Following deep-seated archeological habits, we extrapolate by means of any sort of cross-data that are available, including dating. In this kind of analysis it seems to be particularly difficult to maintain a consistent attitude toward the time factor. Our stage formulations are time-ordered in that they are based on what we know to have actually happened in history, but once defined, are intended to operate without reference to time. This, as we have just pointed out, is not always practicable. Even if it were, we would still

be confronted with a linguistic dilemma. If we do not talk about stages as "earlier" or "later," we have to say "lower" or "higher," thus introducing a value implication which is even more productive of misunderstanding.

The second operating difficulty is related to the first in that it is also the result of insufficient developmental information. This is the fact that our Early Lithic and Archaic stages are almost entirely typological, in the narrow sense of the term, while those for the Formative, Classic, and Postclassic are for the most part configurational. The Preformative marks the beginning of a shift from one set of criteria to the other. Obviously a scheme employing only typological criteria would be easier to operate. Judgments would be based on direct observation of concrete archeological facts. Unfortunately, such criteria do not provide meaningful stages and, in any case, do not hold for the entire hemisphere, as we have found to our cost. On the other hand, use of configurational criteria has to be largely inferential and therefore somewhat subjective.

Derivative of the first difficulty is the familiar problem of cultural lag in marginal situations. A culture in such a situation will have specific elements that properly belong to a later stage than that represented by its general configuration. This appears to be practically a universal condition in the New World. (Whether this is because we have been especially prone to explain cultural development in terms of diffusion, or because American culture has really worked that way, is outside the scope of the present discussion.) When the elements in question are those that enter into our stage definitions, it becomes difficult to classify the culture. To a large extent the concept of a Preformative stage owes its existence to this particular difficulty. We found so many instances where agriculture and one or more other elements of the Formative had penetrated without effecting any configurational change from the preceding Archaic that it seemed possible that this is a meaningful regularity, and, when we found that this condition of agriculture but no other basic change could under certain conditions last for centuries, it seemed reasonable to call it a stage. The question is immensely complicated by the factor of environmental limitations, as we have had numerous occasions to point out. This difficulty of marginal lag occurs on all levels, but obviously we cannot set up a new stage every time it is encountered. For example, as we have seen, in the North American Plains Archaic elements have been incorporated into cultures whose basic patterns were probably very little changed from Early Lithic times; in lower Central America cultures have taken up, and in some cases even pushed to a higher degree of technical advancement, Classic and Postclassic elements, but otherwise remained in all essentials on the Formative stage; the Formative Mississippi cultures of the southeastern United States owe much of their special flavor to Postclassic developments in Middle America. The answer, of course, is better stage definitions, less reliance on individual elements, more careful analysis and description of total configurations. The latter, if we may be allowed to beat the drum again for our previous paper, can be done most effectively on the level of the phase.

Deriving out of both of these primary difficulties is the simplistic defect of classifying wide varieties of cultures under the same cognomen upon the

basis of limited criteria (frequently technological). This is particularly evident in such a stage as the Archaic. Basing a judgment upon the archeological residue, such cultures as those of the eastern United States Archaic, the California Archaic, and the Brazilian coastal shell mounds show remarkable parallels; yet when we reflect that the archeological residue of the ethnohistoric cultures of the northwest coast of North America would not be out of place in such assemblages—and are, in fact, classified as Archaic under this scheme—we are aware of the great sociopolitical complexity and range of variation that these prehistoric "Archaic" cultures must have possessed. This, of course, is a function of the fact that a total cultural situation cannot be fully reconstructed from its fragmentary and incomplete parts. In the specific confines of a given historical context such reconstructions may achieve a high degree of detail and probability, but on a general analogical basis the reconstructed picture must remain incomplete.

The third difficulty, that concerned with cultural regression, is an interesting one. For the areas of Nuclear America the Postclassic stage is, in some senses, a regression. Many of the artistic, architectural, and intellectual achievements which characterized the Classic stage fade and wither. Yet by other standards an argument could be made out against regression. The problem of establishing criteria for "advance" as opposed to "decline" is fraught with so many subjective perplexities that arguments on this score seem somewhat sterile. The most significant thing is that the Classic and Postclassic stages in Nuclear America were different. Turning to other American areas, the question plagues us in a different and more serious form. For example, in our classification the southwestern and southeastern United States are terminated in their histories on the Formative stage. This gives the impression that they were still on some kind of an upward gradient and tends to obscure their own unique configurations of culture growth and change. There is, in fact, evidence in both areas that certain types of decline had set in well before the appearance of the Europeans. In other words, looked at on a strictly area basis the course of Southwestern culture history seems to show certain contours of rise and fall that are of utmost significance for its internal understanding. It is doubtful if any America-wide historical-developmental scheme can surmount this difficulty.

So much for the "workability" of the scheme as a classificatory device that will illuminate rather than obscure the main courses of New World culture history and development. Turning to our second big question—Does this kind of classification furnish new or valuable insights on general or specific New World problems?—the prospects are much more favorable. We feel that it does. A number of problems are raised and others brought into sharper focus when viewed in the over-all alignments of the classification. All of these are problems of fundamental interest. Briefly, here are several:

(1) The problem of an early percussion chipping technological stage or period in the New World. We have noted that a number of archeological assemblages are characterized by this kind of technology. No clear case

can be made for its temporal priority with relation to early pressure flaked technologies, but there are evidences that it may be just as early in the Americas. Is there such a stage? Is it related to the Old World lower paleolithic?

(2) There is the possibility of a basic continuity from such a percussion chipping stage on into the Archaic technologies and even into the Preformative. Presumably such a continuity would have emphasized collecting and gathering as a subsistence mode, eventually switching to agriculture in some areas. If such were true, the large mammal hunting of the Plains, the North American southwest, and parts of southern South America becomes a specialization slightly off the main line of American prehistory—a specialization that could not have taken place until suitable weapons had been introduced or developed and which tended to diminish and disappear in some areas with changing climatic conditions and the disappearance of large mammals. Such an interpretation leaves the chronological priority of Early Lithic over Archaic in some question. Were the two, in large part, contemporaneous?

(3) What is the nature of "Woodland" culture in eastern North America? The classification which we have presented suggests that it is compounded of a number of boreal influences which began to impinge upon North America in the Archaic and of which cord-marked and fabric-marked pottery are the determinant characteristics.

(4) The concept of a New World Archaic provides a frame of reference for problems of Eskimo-Indian interchange in northern North America.

(5) The concept of the Preformative focuses attention upon the lack of celerity of the "agricultural revolution" in the New World.

(6) The combined developmental-historical point of view highlights the possibilities of Preformative or Formative type developments based upon resources other than agriculture. The highly developed fishing and marine hunting economy of the northwest coast of North America is, of course, a striking example. Another is the Hopewell and Hopewellian cultures of eastern North America. The latter had maize agriculture—at least in some regions—but there are indications that this kind of food production did not have primary importance. This leads directly into another problem.

(7) What were the special conditions surrounding certain advanced Preformative cultures in eastern North America (Hopewellian, "Middle Woodland")? Did Archaic culture have a subsistence adjustment to its eastern woodland environment that was so successful that early maize agriculture was not rapidly assimilated?

(8) Was the challenge of unfavorable environment a significant factor in the development of early maize agriculture? The earliest known Preformative developments are in semiarid regions. Is there an important conditioning factor here?

(9) The Preformative has not yet been discovered in what appears to be the real heartland of New World agriculture and the area of the earliest Formative cultures (southern Middle America). Does this mean that Formative cultures developed very rapidly, even suddenly, in this area?

(10) And, if so, are significant Old World stimuli to be sought for?

(11) On a hemisphere-wide basis we have classified the peaks of cultural attainment in the southwestern and southeastern United States as Formative, not Classic. In relation to this we have advanced the theory that multiple and interacting regional centers on the Formative stage are a prerequisite for subsequent Classic development. Does this theory hold?

(12) For the coast of Ecuador and with, admittedly, very limited information, we have suggested that local cultures of a Formative type were transformed into Postclassic developments as a result of close trade and contact with nearby north Peru. Will archeological facts, as they accumulate, bear this out?

(13) A weakness of the scheme as a rigid classificatory device for cultures—the necessity of depending upon technological criteria—can, at least, be turned into an asset of heuristic value. What limits do technology and environment impose upon cultural development? This is an extensive problem area to be explored rather than one in which causal relationships should be assumed. Even with the limitations which confront us in the use of archeological data much more exploration can be done along these lines.

(14) Finally, and perhaps most important, the classification provides a starting point for an examination of historical interrelationships. The developmental grouping of cultures does not demonstrate historical linkages, but it assembles data into categories in which we may reasonably seek interrelationships.

Our third general question—Does the scheme offered here facilitate comparisons between Old and New World archeology?—is beyond the scope of this paper. The classification was not designed with that object in view, therefore the fact that certain rough equivalences do exist is interesting and probably significant. Such very imperfect analogies are stronger on the lower levels of the classification than on the upper. It will be recalled that we referred to our Preformative stage as transitional in a methodological sense. It is also transitional in another and more significant way. It is the turning point in New World culture history. The first two stages, Early Lithic and Archaic, are not only based on technological criteria, chiefly lithic, but are also reflective of actual historical connections with the Old World. We will not say too much about the putative stage of stone technology that is analogous to the lower paleolithic cultures of Southeast Asia because we were unable to verify it as a stage separate from Early Lithic, but, apart from this, our Early Lithic is essentially analogous to the north Eurasiatic upper paleolithic and

our Archaic to the mesolithic and improverished neolithic of the same area. While fully aware of the hazards of such comparisons, it seems to us that they are not entirely meaningless. This is after all the period of migration and settlement, and it would be very strange if the general succession of cultures in the New World bore no relation to that in the Old. This pale reflection of Old World sequence in North America is still paler in Middle and South America, hence the difficulty of applying our stage definitions to the lithic complexes in those areas. In short, the Early Lithic and Archaic reflect a general situation of marginal dependence on the Old World. We can more or less successfully interpret the technologies of these stages from what we know of their Old World analogues. For certain manifestations, notably the microcore and blade tradition, or the Woodland pottery tradition, we can go further and posit specific Asiatic origins. With the Preformative, however, we begin to reckon with elements that have no specific Old World parallels. The crucial importance of this stage as representing the assembling of specifically American culture which crystallized in the Formative can hardly be exaggerated. From here on, Asiatic influences may have continued to filter through from the north, or directly across the Pacific, but these influences are never again paramount except on the periphery of the North American continent. Leaving aside the important but unresolved question of whether or not trans-Pacific influences were significant forces in the rise of American agricultural civilizations, there can be no doubt that the Nuclear American centers (Middle America to Peru) were culturally dominant in the later stages of New World development.

Hence, the present scheme brings into alignment certain Old and New World parallels on the Early Lithic and Archaic stages which are almost certainly of historical significance. At the same time, this maintenance of contact with history, which is an essential part of the scheme, works against a correlation on the later stages. For such a correlation we would require either a demonstrated case for late Old World–New World contacts or a totally different approach, one that is based on a causality that is inherent in culture development, if such a thing exists. This problem is entirely outside the scope of the present study.

As a final evaluation of our historical-developmental scheme for New World prehistory, we sum up by saying that it has serious classificatory defects. Its adherence to the broad outlines of historical reality gives it a certain value as a recapitulation of American archeology, but the scheme does not provide a short and simple answer to New World culture history and process. Our intent has been to examine and evaluate within the framework of a hypothesis. In the course of this examination a number of interesting problems have been highlighted. It is to be hoped that in the presentation we have advanced these will draw the interest of our colleagues.

NOTES

[1] A good discussion of confusion between developmental stage and chronological horizon is found in Kluckhohn and Reiter 1939.

[2] Pedro Armillas has recently prepared a synthesis of New World culture history for the *Program of History* of the Pan-American Institute of Geography and History (ms. of September 1954).

[3] Steward (1949a:8) lists them as: (1) Hunting and Gathering; (2) Incipient Agriculture; (3) Formative; (4) Regional Florescence; (5) Initial Conquest; (6) Dark Ages; (7) Cyclical Conquests; (8) Iron Age Culture; (9) Industrial Revolution. Stages 1 and 2 parallel his first classification. Stage 3 combines 3 and 4 from the first classification. Stage 4 is the same as stage 5 in the first classification. Stages 5, 6, and 7 parallel stage 6 of the first classification. Stages 8 and 9 are beyond the range of New World civilization.

[4] Actual radiocarbon dates are as follows: 7932±350 B.C. for Folsom at the Lubbock site in northwestern Texas (Sample No. C-558, Libby 1952a); 5122±300 B.C. for Plainview and Long at the Allen site in southwestern South Dakota (Sample No. C-604, Libby 1952a); 4968±500 B.C. and 4225±250 B.C. for Eden-Scottsbluff at the Horner site in northwestern Wyoming (Sample Nos. C-795, Libby 1954a; C-302, Libby 1952a). Dates at the Ft. 41 and Ft. 50 sites in the Medicine Creek locality, Nebraska, appear to give a greater antiquity to Plainview and Scottsbluff than that indicated above, but the final report on the stratigraphy and associations of these sites has not yet appeared.

[5] Among early radiocarbon dates from cave sites in the Great Basin are the following: 7102±350 B.C. at Fort Rock Cave in southern Oregon (Sample No. C-428, Libby 1952a); 9502±600 and 9200±570 B.C. at Danger Cave in the Great Salt Lake Basin, Utah (Sample Nos. C-609, C-610, Libby 1952a); and 7838±630 B.C. also at Danger Cave (Sample No. C-611, Libby 1952b).

[6] Radiocarbon dates for the Denbigh flint complex are 1546±230 B.C. and 2705±220 B.C. (Sample Nos. C-792 and C-793, Libby 1954a). A related phase at the Trail Creek site gave a date of 4042±280 B.C. (Sample No. C-560, Libby 1952a). This phase is thought to be somewhat later than Denbigh (Collins 1953).

[7] Radiocarbon dates from two sites of the Green River phase in Kentucky are as follows: Annis site—5423±500, 3198±300, 2949±250 B.C. (Sample Nos. C-180, C-116, C-251, Libby 1952a); Indian Knoll site—3758±350, 2943±560 B.C. (two runs on Sample No. C-254, Libby 1952a). The sample dated 5423±500 from Annis was from a higher level in the midden than the other two samples dated and is therefore considered to be in error (Webb, in Johnson 1951:30).

[8] Old Copper, until recently little more than a technological tradition, has now become one of the most intriguing cultures in eastern North America. Radiocarbon dates recently obtained an antiquity of 3646±600 B.C. and 5556±600 B.C. (Sample Nos. C-836, C-837, C-839, Libby 1954b), which, if substantiated, would make this the earliest use of metals in the Americas and one of the earliest anywhere in the world.

[9] This is by no means a new idea. The suggestion made by Sears (1948) that Archaic is merely Woodland without pottery arose from the same observation that we have made above, to wit, that the addition of pottery had very little effect on the pattern as a whole. We are simply turning his statement around to read: Woodland (early) is merely Archaic with pottery. Fairbanks was expressing the same idea in pointing out that Early Woodland cultures in the Southeast are in effect merely transitional from Archaic to Middle Woodland and that no change in basic economy was involved (1949:59).

[10] The Signal Butte I horizon is dated at 1495±120 B.C. (Sample No. L-104A, Kulp, Feely and Tryon 1951). This date is far later than previous archeological estimates.

[11] Cf. note 1 above.

[12] The difficulty of generalizing about Texas archeology is increased by the use in that area of McKern taxonomic nomenclature to express chronological relationships. A Texas "aspect" is in effect a major regional period. A "focus" may be equivalent to the original definition in McKern methodology with a relatively short time dimension—in other words, what we would call a "phase" —or it may be coterminous with its aspect, with a time dimension of several millennia.

[13] Cochise dates are as follows: 5805±370 B.C. and 4259±450 B.C. for Sulphur Spring (Sample Nos. C-216 and C-511, Libby 1952a); 2055±270 B.C. for Chiricahua (Sample No. C-515, Libby 1952a); and 512±310 B.C. and A.D. 189±430 for San Pedro (Sample Nos. C-519 and C-518, Libby 1952a).

[14] Radiocarbon dates associated with the lowest culture-bearing levels at Danger Cave are as follows: 9502±600 B.C., 9200±570 B.C. (Sample Nos. C-609, C-610, Libby 1952a); 7838±630 B.C. and 7009±340 B.C. (Sample Nos. C-611, C-640, Libby 1952b).

[15] This early date for Bat Cave corn is supported by the date of 2494±280 B.C. for vegetal material from a cornbearing level in Tamaulipas (Sample No. C-687, Libby 1952a). According to verbal information from Dr. Mangelsdorf, the Tamaulipas corn is far less primitive than the earliest Bat Cave corn, so, if the Tamaulipas date is correct, 3000 B.C. is a reasonable extrapolation from the Bat Cave dates.

[16] Earliest Basket Maker II tree-ring date A.D. 46 (Morris and Burgh 1954:48).

[17] If we accept Gladwin's revised dating (1948) that compresses the whole Pioneer period at Snaketown between A.D. 750 and 900, this statement, of course, becomes nonsense.

[18] Radiocarbon dates for Adena in Kentucky are 699±170 B.C., 215±175 B.C. (Sample Nos. C-759, C-760, Libby 1954b). Dates recently published from Adena mounds in Ohio are 423±150 B.C., 826±410 B.C., and A.D. 529±250 (Sample Nos. C-923, C-942, C-874, Libby 1954b). Ohio Hopewell dates are 334±210 B.C., 93±250 B.C., and A.D. 0±200 (Sample Nos. C-137, C-139, C-126, Libby 1952a). The Ohio Hopewell dates are supported by several Illinois Hopewell dates which are in the same order of magnitude.

[19] The stylistic affinities between Weeden Island and some of the Tlatilco pottery (Porter 1953) is too close to be fortuitous, notwithstanding the considerable discrepancies in present dating estimates.

[20] This date is an average of two runs on charcoal from Poverty Point phase deposits in the Jaketown site, near Belzoni, Mississippi. Poverty Point materials on the Jaketown site were identical to those from the type site in Louisiana (Ford, Phillips and Haag 1955).

[21] This date was obtained on vegetal material from the same level that yielded corn. Cf. note 1, above.

[22] The date of 1359± 250 B.C. for Early Zacatenco in the Valley of Mexico is consistent with the Valley of Mexico stratigraphy and with the majority of the other Carbon 14 dates (Sample No. C-196, Libby 1952a).

[23] Radiocarbon date of 714±200 B.C. for Coastal Chavín culture in northern Peru (Sample No. 75, Libby 1952a).

[24] This would be true if agriculture and ceramics were introduced into northern Chile and northwestern Argentina on a Derived Tiahuanaco level. It is more likely, however, that these events antedate Tiahuanacoid diffusions from Peru (see Bird 1943).

[25] An area to be defined approximately as Kirchhoff (1943) defines "Mesoamerica."

[26] If this statement appears to be at variance with the conclusions previously expressed by one of the present authors (Phillips, Ford and Griffin 1951:451), it is because we are now attempting to take a more precise view of Mississippi culture. We now prefer to think of some of the "centers" of Mississippi development, referred to in the publication cited, in terms of Mississippi influence upon cultures that in the main belong to other cultural traditions, e.g., Etowah-Lamar, Fort Walton, Plaquemine, etc. Such a view permits us to speak of a nuclear area for the Mississippi tradition.

[27] One of the puzzling anomalies of Eastern archeology is the fact that dating estimates are as incongruent and unreliable in the later periods as in the earlier, if not more so.

[28] Fort Ancient is perhaps not a good example. The earlier phases (Baum) show a mixture of Woodland and Mississippi elements but the later Madisonville phase is practically all Mississippi. Nevertheless, it differs in essential respects from the nuclear Mississippi tradition as considered here.

[29] Rouse (1953) has emphasized the distinction betweeen the cultural climax Taino phases of the West Indies, referring to these as "Circum-Caribbean" in type, and the earlier agricultural phases, which are designated as "Tropical Forest." This distinction has validity in the West Indian sequences, but in the present paper we are grouping both as "Formative."

[30] The 12.9.0.0.0 correlation, favored by Spinden, would place this date 260 years earlier. Recent radiocarbon dates (Kulp, Feely and Tryon 1951; Libby 1954a) have favored the 12.9.0.0.0 correlation.

[34] There is considerable debate as to whether Middle Tres Zapotes-La Venta is contemporaneous with, or earlier than, the Tzakol phase of lowland Maya.

[35] See dates on Mochica and Nazca in Libby 1952a.

[36] The term "Expansionist" has been used for this stage (Willey 1950).

[37] Schaedel (1951) has distinguished between "urban elite" and "urban lay" centers or cities, defining the former by the presence of ceremonial, political, or palace architecture and the latter by the absence of these features. However, in these cases, the "city" of a purely "lay" nature was within the confines of the same small valley as an "elite" center, and it appears as though both were integrated parts of a "total community" even though they were architecturally distinct and separated from each other by a few miles.

[38] The term "Militaristic" has been used for this stage (Armillas 1951).

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A Study of the Southwestern Fiesta System as Exemplified by the Laguna Fiesta[1]

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FIESTAS, with Catholic masses and processions, markets, carnivals and/or native dances, honoring the patron saint of the community, are found throughout the Catholic countries of Latin America. This fiesta pattern extends northward into the area of early Spanish occupation in the North American Southwest where it still appears in the annual calendar both in Spanish-American and in Indian villages, notably among the Eastern Pueblos in New Mexico and the Yaqui and Papago in southern Arizona.

Unlike the more famous Southwestern "fiestas" or "ceremonials," which were initiated, or are now promoted, by Anglo-American businessmen, the village fiestas are predominantly "native" affairs. The Gallup Inter-tribal Indian Ceremonial and the Flagstaff Pow-Wow, initiated and managed by Chambers of Commerce (and derivative organizations), are examples of the "ceremonial" for which Indians are hired to perform for the Anglo tourists whose presence in the town promotes the local business interests. The Santa Fe Fiesta has also tended to become a commercialized Chamber-of-Commerce-type event. The village fiestas, on the other hand, have deep roots in history and important relationships to the Catholic religious calendar and the annual cycles of tribal ceremonies. The Laguna Fiesta exemplifies this village-type fiesta, for it is controlled and participated in by "native" Southwestern peoples, with Anglo businessmen and tourists playing relatively minor roles.

In the present paper I shall describe and analyze the Laguna Fiesta as an instance of the village-type fiesta system that is characteristic of the contemporary Southwestern cultural scene and contains a number of significant properties that have not been heretofore fully treated in the anthropological literature.[2] It is, in the first place, one kind of stable subsystem which, for purposes of analysis, can be lifted out of the total Southwestern social system composed of Indian, Spanish-American, and Anglo-American cultural groups, just as other subsystems, such as Laguna Pueblo, the Navaho tribe, or a Spanish-American village, can be described as entities. As a type of identifiable subsystem, the fiesta is characterized by diverse cultural components, by periodicity in operation but remarkable continuity in structure, and by an inner coherence and integration that depend upon *situational adjustments* among segments of the diverse cultures rather than upon *stable, permanent changes* in their value systems.

In the case of the Laguna Fiesta, segments of several cultural groups become articulated into a subsystem that is activated for only two days each year. But despite this periodicity in operation, the several cultural groups can be seen as having complementary, although differential, roles, expectations,

and images of the fiesta that weld the intercultural relationships into a remarkably stable subsystem within the total social system of the Southwest. The points of intercultural articulation in this subsystem are, for the most part, based upon mutual economic and recreational adjustments to the fiesta situation rather than upon a sharing of basic social and religious values. The Laguna Fiesta thus provides in microcosm an understanding of some aspects of the multicultural structure of the Southwest and of some of the important processes of acculturation which had their beginnings in early contacts among Navaho, Pueblo, and Spanish and are continuing in the present scene. The acculturative phenomena highlighted in this fiesta system are the close linkages between changes in mode of transportation and shifts in hospitality patterns, the persistence of cultural form while cultural content is modified, and the type of intercultural adjustment through cultural pluralism in values which characterizes Indian-Spanish relationships in the Southwest.

HISTORY OF THE LAGUNA FIESTA

The pueblo of Laguna lies in the San José Valley some forty-five miles west of Albuquerque, New Mexico. It was founded in 1697 or 1698 by Keresan-speaking peoples from the Rio Grande, as a result of the disturbances brought about by the Spanish reconquest of New Mexico after the Pueblo Rebellion of 1680 (Eggan 1950:253). The Catholic mission was established in 1699 (Prince 1915: 203), and in the same year the new village was visited by Governor Cubero and named San José de la Laguna. The name "Laguna" was derived from the fact that the village was built on a rocky knoll overlooking a lake formed by lava dams on the nearby river. The designation "San José" established St. Joseph as the patron saint whose "day" is still celebrated by the annual fiesta.

Some time after 1850, as the threat of Navaho and Apache attacks diminished and irrigation projects on streams flowing down from nearby Mount Taylor became possible, the Laguna population began to disperse into some nine or more farming villages. At present the population is approximately 3,000, but the main village is largely deserted except on the occasions when the people of the farming colonies return to participate in ceremonials.

Located on the east-west line of travel between the Rio Grande and the pueblos of Zuni and Hopi to the west and within the southeastern sector of Navaho occupation, Laguna has long occupied a pivotal geographical position with respect to the movement of and contact between the various peoples of New Mexico. Sustained Anglo-American pressures for acculturation began with the arrival of a Baptist missionary in 1851 and increased with the appointment of a government teacher in 1871. The government teacher and his brother both married Laguna women and later served terms as governor of the pueblo. In 1875 a Presbyterian mission was established and is still active (Crane 1928:66–67; Parsons 1939:888). Meanwhile, tracks for the Atlantic and Pacific Railroad (now the Santa Fe system) were laid past the pueblo in 1882, and the route was later followed by U. S. Highway 66. The diverse cul-

tural origins of the inhabitants, the presence of a number of missionaries and government teachers in the early days, and the contacts brought about more recently by the construction of the transportation arteries near the pueblo have led to more rapid acculturation at Laguna than in any other Southwestern pueblo.

Throughout this 250-year history the Laguna Fiesta has continued as an important annual event in the ceremonial calendar. Originally the fiesta for San José was celebrated on March 19, but later the date was changed to September 19 "because the ideal September days were more suitable for pilgrimages, and also because there was a plentiful harvest at this time" (Dutton and Marmon 1936:15; see also Goldfrank 1923:188). Otherwise, the basic religious patterns of the fiesta have persisted, unaltered (except in cultural *content*) by the influence of the Protestant missions, the schools, the government agents, and the tourists.

There have been changes in the nonreligious aspects of the fiesta, perhaps the most important of these having taken place between 1915 and 1920 when Philip T. Lonegran, the Indian agent, ". . . seized upon this Laguna holiday as the time and place for an annual Indian fair, to ensnare with its exhibits and premiums and sports the combined interest of all the people" (Crane 1928:39). The fiesta had been of importance earlier as a meeting place for trade and recreation for both Indians and Spanish-Americans, but with the active promotion by the Indian Service, the economic and recreational aspects were emphasized and elaborated. By the time that Crane (1928:34–58) described the fiesta in 1919 and Goldfrank (1923:188–95) in 1921, the event had all the characteristics of "a county fair."

PRESENT STRUCTURE OF THE FIESTA

An observer's first impression of the fiesta is that thousands of people move rather aimlessly through a welter of activities during the two days of festivities. But after continued observation and systematic interviewing over three or four successive seasons, the initial impression gives way to an understanding of the fiesta situation as a highly structured series of events. The fiesta is at one and the same time: (*a*) a religious ceremonial which combines Catholic vespers, masses, and processions honoring St. Joseph, with a Laguna Harvest Dance in the plaza; (*b*) an enormous "native" market for the exchange of economic goods; and (*c*) a carnival with merry-go-rounds, tent dances, bingo and other games of chance, hot dog and hamburger stands, etc.

At present it is estimated that over 5,000 people participate in the fiesta. The main cultural groups represented, in order of numbers attending, are as follows: Navahos (predominantly from parts of the Eastern Navaho area); Lagunas; Acomas; Spanish-Americans; Anglos; Zunis; Eastern Pueblos.[3]

OUTLINE OF MAIN EVENTS

The total fiesta is characterized by the events or phases which are briefly described in the following sections and compared with the same events as recorded by Crane and Goldfrank.

(1) *Preparations for and travel to the fiesta*

Most Laguna families move in from the farming villages in the week preceding San José's Day. As "hosts," they prepare for the fiesta by whitewashing the inside and plastering the outside of the church, cleaning the floor and repairing and plastering the walls of the plaza, and cleaning and plastering their houses. There is singing and dancing practice for the Harvest Dance, and food preparation which includes baking bread in great quantities, roasting corn, and butchering sheep and cattle.

Meanwhile, the "guests" are also making preparations. The Navahos butcher sheep, weave rugs, and make jewelry to trade at the fiesta. Hill (1948:382) indicates that Navahos are cognizant of Pueblo ceremonial calendars and use this knowledge to full advantage in planning trading expeditions. Traditionally, Navahos also engaged in trading ritual consisting of prayers and songs (Hill 1948:382–84); today they still think of the dangers of being bewitched and carry "gall medicine" as a protection against witchcraft at the fiesta (Kluckhohn 1944:27). Some Navahos also visit a "curer" in Acomita for witchcraft cures at this time. The Zunis make jewelry to sell, and the more devout say prayers and sing sacred songs the night before they leave for Laguna. The Spanish-Americans from the Rio Grande Valley pick fruit to sell; a few from the nearby villages make plans for selling liquor to the Indians who attend the fiesta. Precautions against possible witchcraft are also taken by Spanish-Americans, who wear crosses and "rosarios" (containing the image of the Virgin Mary and Infant Jesus) to ward off witches. Families in the Eastern Pueblos prepare turquoise (especially in Santo Domingo), chile and melons (especially in Isleta), and other goods to sell or trade.

For weeks in advance, Anglo ranchers, farmers, and traders in western New Mexico are under increasing pressure from their Navaho employees for "time off" for and transportation to the fiesta. Many prepare to haul truckloads of Navahos to Laguna in order to insure their return to the job.

The Anglo and Spanish-American concessionaires who bring the merry-go-rounds, bingo games, etc., to Laguna operate on fixed annual schedules, moving from one fiesta to another throughout New Mexico, and they plan their arrival at Laguna for the afternoon of September 18.

For all cultural groups, travel to the fiesta is now almost entirely by automobile. On the way to the main pueblo of Laguna the incoming "guests" stop in the farming villages at the homes of their Laguna or Acoma "host-friends," leave gifts (Navahos leave mutton, rugs, or jewelry; Zunis leave jewelry; Spanish-Americans leave yard-goods, soap, or towels; and Isletas leave chile, melons, or other foods) and often spend the night of the 17th with them before proceeding on to the fiesta grounds.

The most striking change in this phase of the fiesta is in the mode of transportation. In Crane's photograph (1928:facing 36) of the fiesta scene, presumably taken about 1919, only one automobile is pictured among the wagons; Goldfrank (1923:188) mentions only horses and wagons in 1921. In 1952 there were only three wagons, all driven by Navaho families, and a few Navahos

on horseback; everyone else used motorized transportation. An Anglo automobile dealer was even selling trucks at the fiesta.

Automobile transportation and better roads have made the journeys to Laguna—which formerly took days—possible in a matter of hours. The result is that there is less tendency to spend the night at the houses of Laguna and Acoma "host-friends" en route and more tendency to make the trip from home in one day. There is a consequent decrease in the economic responsibility of the "host-friends" who used to take care of livestock (hay and pasture) as well as house-visiting people. Now they provide for the visitors but have no responsibility for their automobiles.[4]

(2) *Afternoon market and carnival—September 18*

By late afternoon on the 18th, the market and carnival aspects of the fiesta have been set in motion, and the physical arrangements take the form indicated in Figure 1. It is significant that a *definite pattern* emerges which has important relationships to (*a*) the location of the religious structures, i.e., the Catholic church and the Laguna Plaza, which are linked ceremonially with the processions on the following day; (*b*) the geography of Laguna Pueblo, and particularly the fact that roads leading into the center of the pueblo are convenient locations for the lines of concession and peddlers' stands; and (*c*) the economic and social position of various cultural groups: the Anglo, Spanish-American, and Pueblo concessionaires purchase permits and are assigned locations near the centers of religious activity while the peddlers in the market do not buy licenses but move into location (with their products and camps) more informally along the roads leading out to the highway. The Navahos occupy the most peripheral position with their camps ranging at some distance from the roads both to the northeast and northwest of the center of the pueblo. The Pueblos and Spanish-Americans invariably set up tents or sleep in the back of tarp-covered pickups or trucks; the Navahos camp out in the open.

Trading activities begin as soon as families from different groups arrive. Action is always initiated by the Spanish-Americans and Pueblos, who make the rounds of the Navaho camps, selling or trading their products. The Navahos, for the most part, stay close to their camps and wait passively for the various peddlers to arrive. There is an understandable urgency on the part of the more enterprising Spanish-American and Pueblo traders to dispose of their products (particularly the perishable items) as early as possible, for the Navahos have more cash and more goods when they first arrive at the fiesta and are willing to pay more on the first afternoon than later on. Bread or melons, for example, which sell for 25 cents on the first afternoon, can be purchased in the market for 10 cents at the end of the fiesta. Later in the afternoon the peddlers extend their activities into the concessions area, which is not as lucrative for them, however, since they are competing with hamburger stands, bingo games, and a multitude of other items.

Before sundown many of the wandering Pueblo and Spanish-American peddlers have established their combination camps and stands along the roads

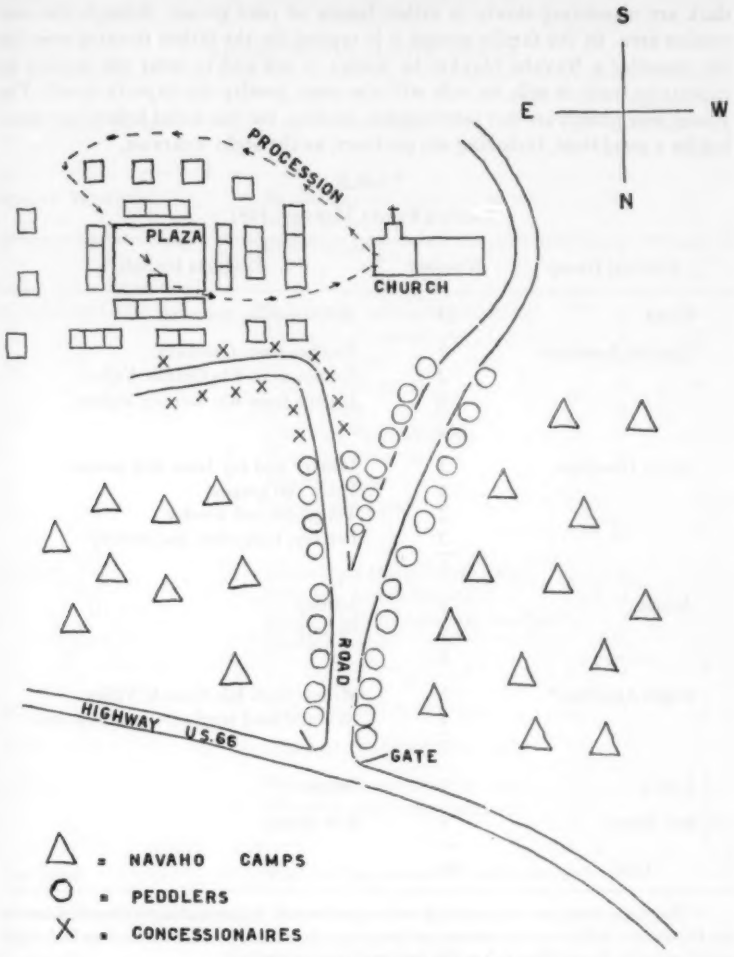

FIG. 1. Laguna Fiesta (schematic).

leading from the highway into the pueblo. These camps are designated as "peddlers" in Figure 1. Although it was impossible to count the various wandering peddlers, a complete tabulation, for 1952, of those who spread out their wares in a fixed location is given in Table 1.

By sundown the Navahos, who during the afternoon have waited passively in their camps for the parade of peddlers, begin to take more initiative in the trading process. They move more cautiously into the fiesta situation but by

dark are wandering slowly in either family or peer groups through the concession area. In the family groups it is typical for the father to carry over his left shoulder a Navaho blanket he wishes to sell and to wear the jewelry he expects to trade or sell; his wife will also wear jewelry she expects to sell. The young peer groups are not interested in trading, but (as noted below) are looking for a good time, including sex partners, as the night wears on.

TABLE 1

LAGUNA FIESTA MARKET, 1952

Cultural Group	Number	Products for Sale
Isleta	23	Melons, chile, tomatoes
Spanish-American	1	Peaches from Cebolleta
	2	Grapes from Rio Grande Valley
	8	Melons from Rio Grande Valley
	11	
Santo Domingo	1	Jewelry and toy bows and arrows
	1	Pears and grapes
	2	Turquoise and jewelry
	3	Pottery, turquoise, and jewelry
	7	
Acoma	1	Jewelry
	4	Pottery
	5	
Anglo-American*	1	Melons from Rio Grande Valley
	1	New and used trucks from Albuquerque
	2	
Jemez	1	Grapes
San Felipe	1	Red sashes
Total	50	

* The Anglo-American who sold fruit was a candidate for Valencia County Commissioner on the Republican ticket and was passing out campaign cards to all his customers. Note that Anglo participation in the peddling role in this market is relatively minor.

By late afternoon the concessionaires have made their arrangements with the Laguna Tribal Council, which requires formal permission to operate concessions and the purchase of a permit costing at least twelve dollars, and have set up their stands to provide food and amusement for all comers. The number, type, and operating group of the concessions for 1951 and 1952 are given in Table 2. Note that although the number has remained almost constant, there are distinctly fewer Anglo and correspondingly more Pueblo concessionaires, especially local Lagunas and Acomas. The recent decrease in Anglo con-

TABLE 2

LAGUNA FIESTA CARNIVAL, 1951–52

Cultural Group	Type	Number	
		1951	1952
Spanish-American	Hamburger and soda pop stand	11	13
	Bingo game	1	1
	Roulette game	1	—
		13	14
Anglo-American	Hamburger and soda pop stand	3	1
	Steam shovel game	1	2
	Shooting game	4	—
	Rolling train game	1	—
	Bingo game	2	—
	Throwing baseball game	1	—
	Western movies in tent	1	—
	Roulette game	—	2
	Selling name pins	—	1
		13	6
Laguna	Hamburger and soda pop stand	2	8
	Soda pop and candy stand	1	—
	Jewelry and children's clothes stand	1	—
	Jewelry stand	—	1
		4	9
Zuni	Soda pop and candy stand	1	—
Isleta	Hamburger and soda pop stand	1	—
Acoma	Hamburger and soda pop stand	—	6
	Ice cream and soda pop stand	—	1
			7
San Juan	Stand selling records of Indian music	1	1
Spanish-American manager with Laguna Indian orchestra	Dance in meeting house (Men pay 10¢ a dance)	1	1
	Total	34	38

cessionaires is the result of the Laguna Tribal Council's higher fee for permits. For example, the merry-go-round and Tent Dance which appeared in 1947 and 1949 were not present in 1951 and 1952. The elimination of the Tent Dance has resulted in an additional important mingling of cultural groups. In the earlier years the Spanish-Americans danced in the large tent, while the young Lagunas danced in the meeting house; now both dance in the meeting house.

The music and dance steps have traditionally been the same (Spanish-American styles), but the participants used to be divided by cultural group between the two dances. Despite the geographical conjunction of the two dances, the social division remains, for dancing partners are not chosen across cultural lines. Note also that although the Navahos are by far the most numerous group at the fiesta, they set up no concessions or stands in either the carnival or market place.[5]

It is evident from a comparison with the earlier accounts that there has been little change in the market and carnival aspects of the fiesta. Both Goldfrank (1923:188–89) and Crane (1928:43–46) mention trading involving Navahos, Pueblos, and Spanish-Americans. They also describe merry-go-rounds, dances with orchestras, games of chance, and stands where cold drinks, ice cream, melons, and oranges were sold. No mention, however, is made of the sale of hamburgers, hot dogs, etc., and it may be assumed that an important shift has taken place in the logistics of feeding the visitors. For while some visitors are still fed in Laguna homes, there is obviously less adherence to this pattern and more use of the commercial hamburger stand arrangement.[6]

(3) *Evening vesper service—September 18*

While the market and carnival continue almost without interruption for two days, the religious aspects of the fiesta are divided into a series of definitely spaced events. The first of these is the evening vesper service which begins with the ringing of the Catholic church bells at 7:30 P.M. on the 18th. In 1951 and 1952 only some 50 persons attended the service, including 12–15 Spanish-Americans, 5–8 Eastern Pueblos, and 30 Lagunas. The Navahos and Zunis were conspicuously absent; the only Anglo-Americans present were the priest and several nuns. Special prayers are said for St. Joseph, and after the service the priest announces that it is time for confession. In each of these years (I did not attend the service in 1947 or 1949) only a few Lagunas went forward to confess.

(4) *Evening market and carnival—September 18*

During the evening, the market and carnival continue along much the same lines as in the afternoon, but there is a noticeable increase in the size of the crowd and a number of new emphases are apparent. Additional Spanish-American families arrive for the evening dance. The younger unmarried members dance in the meeting house while their parents (who inevitably accompany the girls as "chaperones") sit on benches outside and visit with one another and with passing Indian friends. Navahos who have been working during the day in the nearby Bluewater carrot fields begin to arrive in large trucks provided by the farming companies, who send along one or more Anglo foremen to "look after" their labor force.

After dark the opportunities for acquiring liquor are markedly increased, and Navahos begin to drink heavily. The ultimate sources of the liquor are

the bars in Spanish-American villages along the highway, but most liquor which actually changes hands at the fiesta is brought in by Navahos who purchase a supply from Spanish-Americans and resell it to other Navahos at Laguna.

As Navaho drinking increases, the Indian police force, supervised by a special officer from the Indian Service, begins to make arrests. Intoxicated Navahos are locked up in the "Navaho jail," a small room next to the meeting house. During the 1951 fiesta a group of Navaho prisoners happily sang Enemy Way songs inside the jail, pounding on the door of the jail as a drum. It is significant that this is defined by every cultural group as a "Navaho jail," for everyone expects the Navahos to get drunk, be put in jail for the night, and to be tried and fined ten to twenty dollars by the Laguna Tribal Council the following day. I have observed many young Spanish-Americans, Lagunas, Acomas, and Zunis who were as drunk as the Navahos, but in actual practice only Navahos are arrested. The only exception was in 1952 when a Sandia Indian was arrested for bootlegging. When questioned, the Indian Service special officer reported that the Indian police have the authority to arrest anyone who becomes drunk and disorderly. However, *in fact* only Navahos are arrested. It should be emphasized that this procedure is not so much a matter of discrimination as it is a matter of expectations. Everyone expects the Navahos to get drunk, the police watch for Navaho intoxication more than for other disorder, and the whole process becomes an instance of what Merton calls the "self-fulfilling prophecy" (see Merton 1948).

As the evening progresses, the younger Navahos begin to seek sex partners. This procedure is also highly patterned. A group of two or more Navaho girls will walk up and down the road by the concession stands until they attract the attention of two or more young Navaho men. Then the girls walk slowly—with the boys following at a discreet distance behind—down the road toward the highway and turn off into the darkness of the bushes west of the pueblo. Other cultural groups also seek sex partners on fiesta nights, but the procedures are not as highly patterned. There is evidence of philandering by the Pueblo men, both with women from their own cultures and with Navaho women. Since Spanish-American girls are strictly chaperoned, young Spanish-American males also seek partners in the Pueblo and Navaho groups. The most common practice at present for philandering Pueblo and Spanish-American men is to take girls for rides in cars out to the edge of the pueblo.

After the carnival closes at midnight, a group of Navahos usually gathers either near the stands or out in the camp area to sing Enemy Way songs and hold what is traditionally called a "squaw dance." Recently there has been no dancing, but the traditional singing still takes place from about 2 A.M until daybreak.

The earlier accounts of the fiesta make little mention of the night's activities beyond the fact that much drinking took place. Crane reports (1928:57) that he was always present with a force of policemen to place intoxicated participants in the tribal guardhouse, which for two nights "was filled with those visitors who exhibited the effects of too much faith in John Barleycorn."

(5) *Solemn High Mass—September 19*

The activities of St. Joseph's Day are initiated by a sparsely attended mass at 7 A.M. At 9 A.M. there is Solemn High Mass in the church, attended by approximately 300 people. In 1951 and 1952 the congregation consisted of 60–75 Spanish-Americans; the rest were Lagunas and Acomas, with a sprinkling of Eastern Pueblos—especially Isletas and Santo Domingos (the parish of Laguna includes Acoma and several neighboring Spanish-American villages and has a total membership of about 4,000). There were no Navahos in either year, and no Zunis in 1951, but in 1952 one Zuni couple attended. Each year two Anglo-American tourist families stood at the rear of the church for a few minutes.

The choir is composed of Laguna women led by an Anglo-American nun. Anglo-American priests (four in 1951, three in 1952) officiate during the service, which includes prayers, communion (administered to Lagunas, Acomas, and Spanish-Americans), and a sermon on St. Joseph, delivered in English. The virtues of St. Joseph (respect, kindliness, and justice) are stressed, as well as the fact that St. Joseph is the universal patron of the church "and our own patron here at Laguna." After an offertory, the formal church service ends, and the priest asks the governor and his lieutenants to come forward and "talk to the people." The priests then leave while speeches are made in the Laguna language by the governor and by seven other members of the Tribal Council who also stress the fact that this is the fiesta for St. Joseph and ask the people to prepare to carry the wooden image of the saint in the procession out to the plaza.

The present mass is the same as that described in the earlier reports of the fiesta (Goldfrank 1923:189; Crane 1928:41), with the single exception that the sermon was formerly delivered in Spanish rather than English.

(6) *The procession*

After the speeches, the priests return and sprinkle holy water on the wooden image of St. Joseph. The image is then lifted from its niche by the parish priest and handed to two Laguna women who take turns carrying it in the procession which, after leaving the church, passes south of the plaza and enters the plaza on the east. The governor and his staff lead the way, making symbolic "trails" with their staffs of office; next comes an acolyte bearing a gold cross, followed by the women carrying the wooden image; the choir, which sings hymns about St. Joseph; the main body of the procession, composed of a total of 150–200 Lagunas, Acomas, and Spanish-Americans with a few Santo Domingos and Isletas; and, finally, the priests. A few Navahos, Zunis, and Anglo tourists watch the procession, but do not join in.

Upon arrival in the plaza, the image of St. Joseph is placed on a table at the north side in a bower constructed of poles, covered with cornstalks. The front of the bower is left open and on either side of the entrance is a poplar tree. After a prayer the priests leave, the crowd disperses, and Laguna officials are left sitting with the saint during the rest of the day.

The earlier accounts (Crane 1928:41–42; Parsons 1923:284, photo) describe the same general pattern for the procession, but there have been some changes in details. Formerly one or more bell-ringers preceded the saint in the order of march, and the priests walked directly behind the saint. The procession was accompanied by two black-masked clowns, dressed in "Mexican" clothes, who carried whips and served as policemen. Goldfrank (1923:189–90) states that the procession was composed of the "entire village and their guests"; this is clearly not the case today when only a small percentage take part.

(7) *Harvest Dance in plaza*

Dancing in the plaza begins shortly after the procession (about 11 A.M.) and continues until almost sundown. The Darawee—the so-called "corn dancers" or "harvest dancers"—alternate with a team of younger men who perform a more secular dance. The 20 Darawee dancers (10 men and 10 women) are accompanied by a chorus of 10–12 men with drums. The chorus comes in first, singing, gesturing with their hands, and beating time with their feet. The dancers follow in two long lines, two men abreast, followed by two women abreast. The dance steps and formations are the same as those described by Goldfrank (pp. 191–92). In 1952 the women in the dance formation were still dressed in traditional Pueblo dress, but the men were all in white-style trousers and shirts. The two leading men wore kilts, sashes, fox skins, and bells over their trousers and shirts. Four other men had bells on their legs, and all except two held gourd rattles. Only one man wore a headband, and three wore white-style ties. In the chorus only three men wore headbands; all others had large cowboy hats and all were dressed in white-style trousers and shirts.

The more secular dance was formerly a Comanche dance (Goldfrank 1923; Crane 1928:36; Dutton and Marmon 1936:15, photo), but is now either a Hoop Dance (in 1949), an Eagle Dance (in 1952), or is even omitted altogether (in 1951). The Hoop Dance, performed by a team of six younger Lagunas dressed in red, green, and blue feathers, mirrors, and bells (Oklahoma-Indian-style), was very popular with the Indians in the audience and received loud applause at the end of each performance. There was only scattered applause after the 1952 performances of the two Eagle dancers, who were dressed in U. S. Navy enlisted men's blue uniforms with cotton headpieces, wings of eagle feathers, bells, and moccasins.

During the dancing, gifts of food (principally corn, bread, and green chile) are brought into the plaza by Lagunas and Spanish-Americans and placed in front of Saint Joseph. Later in the afternoon this food is distributed to the crowd by the Laguna officials.

Throughout the afternoon an audience composed of several hundred persons surrounds the plaza. All groups are represented in approximately the same proportion as the totals attending the fiesta, except that there are relatively *fewer* Spanish-Americans and relatively *more* Anglo-American tourists. The Spanish-Americans who participated in the Catholic Mass and procession

have drifted away to other activities. Anglo tourist participation reaches a height at this point with as many as 10 groups of tourists present at one time, but they do not stay long. The typical Anglo tourist watches the dancing for a time, wanders through the concessions to buy cokes and ice cream, and through the market to make a few purchases of pottery or jewelry before leaving. Disappointment is almost always expressed because the Laguna dancers do not wear more of their native costumes.

During the last period of dancing, the Darawee and the more secular dance team perform simultaneously in the plaza. Shortly before sundown the dancers leave, the priest reappears and St. Joseph is returned directly to the church by a very small procession consisting of the priest, the governor and his lieutenants, the women who carry the image, and about forty others—all Lagunas and Acomas. Inside the church, St. Joseph is returned to his niche, and the religious phase of the fiesta ends when the priest leads those present in the Lord's Prayer.

Comparing the present Harvest Dance with the earlier records, the shift from the Comanche to the Hoop or Eagle dance has been mentioned. There has been no change in the women's costumes in the Darawee, but additional acculturation can be seen in the dress of the male dancers. In 1921, although Goldfrank (1923:191) states that "any costume seemed to do for the men," there were still dancers wearing skirts with embroidered pieces down the side, and they all wore silk handkerchiefs around the head, carried rattles, and their hands and faces were marked with paint.

(8) *Evening market and carnival—September 19*

The market and carnival activities, which have continued throughout the day as an adjunct to the religious phases of the fiesta, are again stressed on the evening of the 19th, and there are no changes from the evening before.

(9) *Travel home from the fiesta*

Many people leave the fiesta on the night of the 19th, but the main exodus takes place on the morning of the 20th. The visiting Navahos, Pueblos, and Spanish-Americans finish their trading activities and break camp. Final stops are made at the homes of Laguna or Acoma friends in the farming villages on the way out, and at this time the visitors are fed and given gifts (usually corn, melons, bread or other foods) in exchange for the gifts left at these houses at the beginning of the fiesta. Mutual expectations are expressed to the effect that "we'll see you again next year at the fiesta." This gift exchange pattern takes place within the framework of an institutionalized "friend" relationship (see Hill 1948:389 for a description of this type of exchange between Navahos and Utes and Navahos and Pueblos; see also Vogt 1951:18 for a brief discussion of the relationship between Navahos and Pueblos). The friendships are initiated in various ways. In some cases, especially those involving other Pueblos, members of the two families may have met years ago at an Indian Service boarding school. In the cases involving Navahos, the original contacts

were often made two or three generations ago and the "friend" relationship is maintained by the younger generation through the years. No bargaining is involved in the exchange of gifts; no questions are raised as to amount or quality of the gifts. But controls are present in the pattern, especially in the case of visiting Pueblos, by the fact that the visits will be returned and goods exchanged again when the Lagunas go to fiestas in other pueblos. In the case of the Navaho-Laguna relationship, the Navahos may decide to go to a different house the following year at fiesta time if they do not receive gifts in return that compensate them adequately for the goods they brought. On the other hand, if the Navaho visitors are "stingy" with their gifts, the Laguna hosts will provide less in exchange. In brief, a continuing process of adjustive reciprocity makes the system workable.

VARYING IMAGES OF THE FIESTA

Up to this point I have described the fiesta as seen by the observer—the visible world of objects and actions in the situation. I shall now describe briefly the varying definitions of the fiesta in order to illuminate the differences in meaning given to the objective situation.

For the Lagunas, the fiesta for St. Joseph is the high point in the annual calendar of events. The important religious meanings centered in both the Catholic church and the ancient Harvest Dance are combined with the economic meanings centered in the market place and the recreational values in the carnival. All phases are now important in the Laguna definition of a good fiesta. The Lagunas also feel strongly their responsibilities as hosts to hundreds of visitors. The Spanish-American and Eastern Pueblo visitors are regarded as having many of the same interests in the fiesta, especially since these groups are also Catholic and have similar celebrations in their own villages. There is likewise a strong feeling of solidarity with Zunis, whose customs are similar and whose visits will be returned at Shalako time when Laguna families go to Zuni. The Navahos, on the other hand, are regarded as peripheral participants. There is no expectation that they will attend the church services or that their visits will be reciprocated by Laguna attendance at Navaho ceremonials. On the whole, Navahos are viewed as still being "wild Indians" who live back in the woods and whose presence at the fiesta is valued mainly as a source of economic profit.

The image of the fiesta held by the Acomas and the Eastern Pueblos is similar to that of the Lagunas, since these Pueblos also have fiestas which the Lagunas attend. The Acoma definition is closest to that of Laguna, for the Acomas play the role of host for incoming visitors in the farming villages and are members of the same Catholic parish.

The Zuni image of the fiesta is somewhat different. The fiesta is called either "Dance for the Saint" or "Corn Dance." There is recognition of the fact that a Catholic saint (they do not know which one) is involved, but the stress comes upon witnessing the "Corn Dance" (rather than upon the total religious ceremonial linking the Catholic Church with the dance in the plaza)

and upon trading. The Zunis expect to "trade and watch the Lagunas dance," in the words of a Zuni informant. Younger Zunis describe the fiesta as "a peddling fiesta, where you can get more for your jewelry and make a little profit." The Zuni interpretation of the presence of other groups is that "the other Pueblos come to trade and watch the Lagunas dance, the Spanish to worship the saint and to jitterbug in the dance hall, and the Navahos to court the girls and eat."

In the Spanish-American image the stress is upon the Catholic aspects of the situation (vespers, mass, and procession) combined with a lesser stress on the trade and the recreational aspects, especially the evening dance in the dance hall or tent. In the words of one of the Spanish-American informants:

> It's according to old custom. The saints' days. It has been practiced by the Spanish people as a custom for ages. Always united with each other in the celebration for the saint. I can remember it since I was a boy. I have been at this fiesta every year since I was five years old. This is one of the oldest gatherings and is taken in by all the Spanish villages in western Valencia County. We come to see old friends, to do a little trading, and we always go to vespers and mass. After the procession, the Indians take over and dance in the plaza. Sometimes we watch them, but we are just spectators there. That's their own ritual.

The Spanish-American definition of the presence of other groups in the situation is that other Pueblos come primarily to trade, that the Navahos come to do some trading but also to "roam around, drink and have fun." The Spanish-American response is to joke about the "fun-loving" Navahos whose employers "have to bring them over here in big trucks to be sure they get them back to work again."

The Navahos call the fiesta "Corn Brush Hogan," referring to the bower in the plaza in which the saint remains during the Harvest Dance. The use of this name for the fiesta indicates a recognition that something important to the Lagunas is kept in the bower, but none of the Navahos interviewed knew that a Catholic saint was involved or that it was a saint's day in the Catholic calendar. One informant reported:

> The Navahos call this "Corn Brush Hogan." It means they have a big feast. We say this time of year all these Lagunas have their crops in. All these people got corn, oats, peaches, apples. They all get ripe this time of year. That's why they have a big feast.

For the Navahos, the stress is on the market and carnival aspects of the fiesta —defined as a traditional harvest festival, the date of which was set by the Lagunas long ago to insure success for the crops. As a large trading center, the fiesta is a place at which they can obtain goods not ordinarily available to them; there is an abundance of food of choice varieties (each Navaho informant made a point of providing a detailed list of all the foods he obtained or expected to obtain), and there are objects for personal adornment and for the representation of wealth (e.g., turquoise beads). As a carnival, the fiesta becomes a place

TABLE 3

PARTICIPATION BY CULTURAL GROUP IN LAGUNA FIESTA

Cultural Group	Religious Aspects					Carnival Aspects				Market Aspects			Total
	Catholic			Indian									
	Vespers	Mass	Procession	Harvest Dance	Audience at Dance	Concessionaires	Play Games and Purchase Food	Dance in Hall or Tent	Stand Operators	Trade in Open Market	Gift Exchange	
Laguna	XX	XX	XX	XX	XX	X	X	XX	X	XX	XX	11
Acoma	XX	XX	XX		XX	X	X	X	X	XX	XX	10
Spanish-American	XX	XX	XX		X	XX	X	XX	XX	X	X	10
Eastern Pueblo	X	X	X		XX	X	X	X	XX	XX	XX	10
Anglo	X	X	X		XX	XX	X		X			7
Zuni					XX	X	X			XX	XX	5
Navaho					XX		XX			XX	XX	4
Total	5	5	5	1	7	6	7	4	5	6	6	

Note:
XX—Indicates that participation is proportional to total numbers of a cultural group present at the fiesta.
X—Indicates that participation is proportionately less than would be expected on the basis of total numbers of a cultural group present.

in which to find liquor to drink and girls to sleep with, along with games to play and many and varied novel activities to observe.

Anglo-American images of the fiesta vary according to the five different roles the Anglos play in the situation: (*a*) The Anglo-American Catholic priest views the fiesta as a religious celebration for St. Joseph, but recognizes the persistence of Indian religious elements. He expresses less concern about this than he does about the commercialization and the carnival aspects of the fiesta. He expressed relief when the merry-go-round did not appear in 1951 and 1952. (*b*) The Anglo concessionaires define the fiesta almost entirely in terms of economic gains, with no interest expressed in Indian or Catholic rituals or customs. (*c*) The Anglo-American police view the fiesta as a problem situation in which bootlegging must be controlled and law and order preserved. (*d*) The traders and ranchers who bring Navaho employees express some interest in the Indian rituals and customs, but it is clear that they would not attend at all if they were not concerned about getting their employees back on the job. (*e*) The Anglo tourists define the fiesta as an "interesting" situation, a place to observe the curious rituals and customs of the American Indian.[7]

There are three important common elements in these images of the fiesta: (*a*) it is a large and important trading and market center; (*b*) it is a place to "meet friends" in all the various cultural groups; (*c*) but it is viewed (except by the Anglo group) as a dangerous situation in which there are likely to be witches present in the large crowds. This fear of witchcraft is most marked in Navahos, but is also present in the Pueblo and Spanish-American participants.

DIFFERENTIAL PARTICIPATION IN THE FIESTA

It is evident that both the definitions of and the roles played in the fiesta situation by the various cultural groups are related to important differentials in participation. These differentials are summarized and tabulated in Table 3. Note that while only the Lagunas themselves participate in *all* aspects of the fiesta, the Acomas, Spanish-Americans, and Eastern Pueblos participate at least to some extent in each aspect except the Harvest Dance.

The Anglos, Zunis, and Navahos, on the other hand, participate in only selected and relatively peripheral aspects; the Navahos, who are present in the greatest numbers, not only know less about the religious purposes of the fiesta than any other group, but also participate the least in the total situation.

It is also of significance that all groups participate together only in watching the Harvest Dance, and in playing games and purchasing food in the concessions area; similarly, all groups except the Anglos participate in the trading in the open market and in the gift exchange arrangements. These facts make it evident that the fiesta structure as a whole functions as a unit only in its market and recreational aspects.

CONCLUSIONS

There is clearly no consensus among the seven cultural groups attending the fiesta as to the religious meaning of the celebration and no common set of

sacred values. Rather, the fiesta is articulated as a multicultural system in terms of varying definitions and varying roles played by cultural groups in such a way that the critical functions of trade and of feeding and entertaining a large, diverse crowd are carried out. These mutual role-expectations for fiesta behavior have shown remarkable stability and continuity over a long time span.

Some significant acculturative changes, however, are evident in the comparisons with the earlier accounts of the fiesta. The most impressive economic changes are in transportation and in the logistics of feeding the large fiesta crowds. These two changes are interrelated in that the shift from horse and wagon to automobile transportation has resulted in less dependence of visitors upon hosts for several days of feeding and housing during the fiesta. As adherence to the older reciprocal hospitality patterns between cultural groups declines, there is an increasing tendency for Laguna and Acoma hosts to set up hamburger stands and feed the visitors commercially.

The additional recent change in men's costumes for the Harvest Dance, and the shift from Spanish to English in the sermon during the Solemn High Mass, are traceable to further direct acculturation to Anglo-American ways of life. On the other hand, it is clear that acculturation to Spanish-American customs is important in the secular dance patterns found at the fiesta. In these dances, young Acomas and Lagunas dance with steps and to music that are indistinguishable from those found in the *bailes* held in neighboring Spanish-American villages.

The Laguna Fiesta is essentially a type of cross-cultural system in which there is a lively exchange of economic goods and entertainment services, the system being integrated among the cultural groups in terms of these secular activities, but one in which the participating groups continue to be differentiated from one another on the level of more central social and religious values and meanings. In microcosm it reflects the system of *cultural pluralism* in values (see SSRC 1954), which emerged at an earlier period and still continues as an important feature of the Southwestern cultural area. In this system of cultural pluralism among Indian and Spanish-American cultures, there is an active exchange of secular goods and ideas, and, at the same time, a relatively high degree of respect and tolerance for the separate social and religious values (cf. Tax 1941). It is true that the Navahos occupy a subordinate position within this system (whenever they come into contact with the other groups, as in the Laguna Fiesta), but the situation is, nevertheless, vastly different from those situations in the Southwest in which Anglo-Americans play the management role and are not only engaged in an exchange of secular goods and ideas with Indian and Spanish-American cultures but are also involved in actively promoting their Anglo-American value systems.

NOTES

[1] This paper is based upon four seasons, i.e., 1947, 1949, 1951, and 1952, of participant-observation and interviewing at the Laguna Fiesta. I am indebted to the Rockefeller Foundation

(Social Science Division) for the financial support of this research as part of the Comparative Study of Values in Five Cultures Project of the Laboratory of Social Relations at Harvard University. I also wish to express my appreciation to Ethel M. Albert, W. W. Hill, Esther S. Goldfrank, Herbert R. Harvey, Clyde Kluckhohn, Anne Parsons, John M. Roberts, and Clarissa Schnebli for comments and criticisms.

[2] For descriptions of other Pueblo saint's day fiestas, see White (1930, 1935, 1942). Similar types of fiestas in Arizona and Sonora are described by Joseph, Spicer and Chesky (1949); by Spicer (1940); by Williamson and others (1950); and by Schweitzer and Thomas (1952). Other papers, such as those by Parsons (1925, 1936) and Lange (1952), have provided excellent descriptions of the processions and tribal dances which take place on fiesta days but have not included treatments of the total cultural situation.

[3] It was patently impossible for a single observer to count exactly the total numbers or the numbers of each cultural group participating. The estimate of the total was made by counting the total number of automobiles and wagons present at the height of the fiesta on the afternoon of the 19th of September in 1951, and multiplying by eight—a figure which seems high but was arrived at by counting the number of passengers in a sample of a hundred cars arriving at the fiesta (the Navahos, particularly, travel in very full vehicles). The estimate of numerical order of cultural representation was made by two sample counts: (1) of the numbers walking past the concession stands over the period of an hour; (2) of the numbers in different groups participating in and watching the Harvest Dance in the plaza.

[4] It has usually been assumed that improved transportation increases acculturation among peoples of different cultures in the world, but in the case of the Southwestern scene it might be argued that the reverse has taken place in over-all acculturation. For, while use of the automobile makes it possible for folk peoples in remote settlements to extend their travel range—to shop in the Anglo towns more often or to visit people in distant pueblos—the intercultural contacts are now likely to be more superficial and of shorter duration.

[5] The concessions are a lucrative business; my figures indicate that an average-sized hamburger and pop stand shows a profit of over $200 for the two-day period.

[6] At a Navaho Enemy Way ("squaw dance") held near Tohatchi in August, 1952, the writer noted that Navahos are also beginning to adopt the hamburger stand pattern in providing food for large groups. There were no Anglo traders present selling food or soda pop, but there were more than 15 Navaho-operated stands selling hamburgers along with mutton stew and fried bread.

[7] The establishment of the atomic development centers in New Mexico during the past decade has resulted in an important increase in scientists and intellectuals in the Anglo-American tourist participation in events such as the Laguna Fiesta, the Zuni Shalako, etc. Members of the scientific staffs at Los Alamos, Sandia Base, etc., almost always appear at these Indian gatherings now, and as a general rule are well read and deeply interested in Indian culture.

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*Two Processes of Change in Mandan-Hidatsa Kinship Terminology**

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INTRODUCTION

WE ALREADY have some understanding of the progressive stages in the breakdown of a Crow type kinship system under conditions of social change (Spoehr 1947; Eggan 1950), but we lack sufficient information on the actual processes and interactions involved in the shift from one basic kinship type to another. This paper deals with processes of kinship change in one village of Mandan-Hidatsa Indians living on the Fort Berthold Reservation in North Dakota.

HISTORICAL BACKGROUND

The Mandan and Hidatsa are both small Siouan-speaking tribes with a long history of residence in the Missouri River Valley. Although the Mandan and Hidatsa had different origins, spoke mutually unintelligible languages, and lived in separate villages, they have been in intimate and peaceful association for over two hundred years, with constant cultural borrowings and intermarriage. For the problem to be dealt with here, we may consider the two tribes as one.

Both groups were part of the Prairie earth lodge complex, which included the Chiwere and Dhegiha Sioux and the Caddoan tribes and covered the area encompassed by the Arkansas and Missouri River valleys. A dual subsistence economy of agriculture and hunting was adjusted to an abundant ecological setting in the fertile creek and river bottom lands. Each tribe was composed of a number of independent villages integrated by social and ceremonial ties. The characteristic dwelling was the earth lodge, occupied by matrilocal extended families. The social structure of the Mandan-Hidatsa included a clan and moiety system, matrilineal lineages, an elaborate set of societies, and a Crow type kinship system.

In the early nineteenth century, the two tribes were severely reduced in population by a series of smallpox epidemics. By 1862, the remnants of the Mandan-Hidatsa had consolidated with the Arikara in one village for mutual defense against hostile nomads. With the Indian Bureau allotment program in the last decade of the nineteenth century, the people of the village were scattered into a number of separate and smaller communities on the Fort Berthold Reservation. A group of Mandan and Hidatsa families established

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themselves in one of these communities located on an isolated segment of the reservation between the Missouri and the Little Missouri rivers. The village is called Lone Hill by the Indian people. In 1951, Lone Hill had a total Indian population of 267 individuals forming 48 nuclear families.

THE PROBLEM

In the summer of 1951 and continuing in 1952–53, I studied social and cultural change in Lone Hill village. Starting with an investigation of kinship using the genealogical method I obtained kinship schedules essentially similar to those reported by Morgan (1871:291–382), Lowie (1917), and Bowers (1950, ms.) as characterizing the traditional Crow lineage system. It became apparent that for some members of Lone Hill, both old and young, the terminological system had not changed. While the majority of the schedules gathered in the village represented the Crow lineage system, this was not so for every case. A number of schedules were obtained with English terms and were directly comparable to the American type of system. Thus, within one community, I obtained two kinds of kinship patterns, the first aboriginal Indian and the second modern near-white.

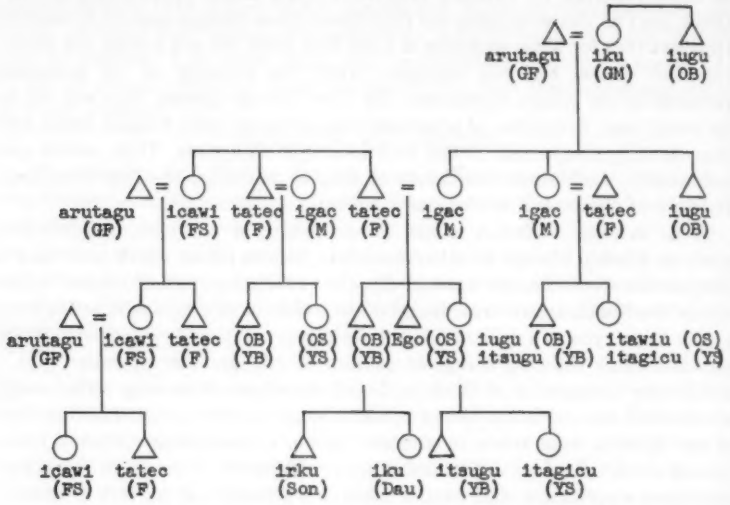
This extreme variation is not too surprising if one considers previous work on kinship change in other American Indian tribes which have had a comparable Crow lineage system. Spoehr (1947), comparing related tribes within the Southeastern area, found direct evidence of a complete series from a pure Crow type to a variant of an American generational system with intermediate forms showing a regular pattern of change. The Schmitts (n.d.), within one community of Wichita, found variations in kinship terminology which fitted into the latter part of Spoehr's more complete series. The direction of the Wichita data tends to validate Spoehr's conclusions—when a Crow system changes under modern acculturative influence, there is an increasing patrilineal emphasis, a shift from a lineal to a generational pattern in classification, and a diminishing importance of kinship in regulating social life.

The point to be stressed here is that the terminological systems gathered among the Indian people of Lone Hill village were either pure Crow or white generation types—both extremes without the intermediate variations. While a generational type kinship system is found among the Mandan-Hidatsa and in the cases reported upon by Spoehr and the Schmitts, the absence of intermediate variations in Lone Hill suggests a significant difference in the kind of acculturation taking place. This difference may be clarified by a description of the types of terminological systems found in Lone Hill, followed by an analysis of the mechanisms of kinship change.

THE KINSHIP PATTERNS

Of the two terminological kinship patterns found in Lone Hill, the pure Crow type remains the operating functional system for the majority of the people of the village. Figure 1 is a diagram of the system in the Hidatsa language with English equivalents. The orthography follows Bowers (ms.).

Although Lone Hill is composed of members of the Hidatsa and the Mandan tribes, the Hidatsa language has become standard in the village while the Mandan language is rapidly becoming extinct. This may have been due to the small percentage of Mandan-speaking people in Lone Hill when the community was formed. The Crow terminology is used in the Indian language in daily face-to-face contact among most of the people in the village. The meanings and applications of the terms have not changed. Bowers (1950:1) notes that "the kinship system has been affected little by recent events." Figure 1 is based upon observations of usage throughout the village and was



(Note—For a female ego, sister's children are son and daughter.)

FIG. 1

checked by formal schedules obtained from 14 individuals: 8 males ages 34 to 70, and 6 females ages 23 to 60.

In certain situations the people of Lone Hill make a direct linguistic translation of the Crow terminology from the Indian language into the English language. Classificatory fathers are called uncle, classificatory mothers and fathers' sisters are called aunt, while niece, nephew, and cousin terms are applied indiscriminately and usually incorrectly from the white point of view. There are no uncle, cousin, niece, or nephew terms in the Indian language. Figure 2 is offered as an illustration—note particularly the descendants from the father's sister. As the diagram shows, the generation principle does not apply.

This use of English is not a variation of the Crow system but is rather the same system with an alternate language. The English kinship terms do not

form a reciprocal system and do not conform to expected white usage, for example, an individual whom you address as uncle may call you cousin or brother rather than nephew. It is an egocentric system and is used only when an individual in Lone Hill finds himself in a situation in which he is forced to speak English. All but two of the 137 adults of the village are bilingual, as they speak English in addition to the Indian language. English is most often learned in government schools as a second language and is used by the Indian people to deal with the outside world: in contact with Indian Bureau employees, local white merchants, or when members of alien tribes visit or marry into the village.

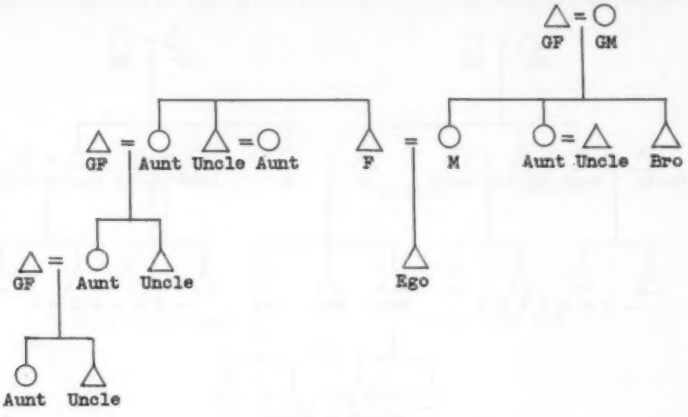


FIG. 2

The second major kinship pattern in Lone Hill consists of cases in which English terms are used in the white generation pattern, as indicated in Figure 3. My observations indicate that the generational system was used only by seven acculturated families. Many of the individuals in these families have spent most of their lives away from the village in off-reservation schools. The system was checked in formal interviews with six people, all between the ages of 20 and 30. In one instance a father's brother was called a cousin rather than an uncle because, as my informant said, "He is too young to be my uncle." The situations in which the generational system is employed tend to be relatively restricted and occur when acculturated individuals relate to one another, their close relatives, and whites in or outside the village.

An additional kinship complication arises in those homes in which a member of an alien tribe—usually Crow, Sioux, Blackfoot, or Arikara—marries a Mandan-Hidatsa resident of the village. No schedule can be given here as there is no general pattern, but one case will be briefly described as an illustration.

As shown in Figure 4, John Bear Leg is the child of a mixed Hidatsa-Arikara marriage. The family lives in Lone Hill and speaks only English at home as Mrs. Bear Leg, an Arikara woman, does not understand Hidatsa. While discussing kinship, John referred to his father's sister's daughter, Mary White Duck, by the term "sister" and told me, "That's the white way." This is incorrect, of course, as in the American way Mary would be a cousin to John, while in the Hidatsa way, she would be an *icawi* or father's sister. I became excited here, as I thought I had found a case of ironing out generation differences which would represent an internal shift in kinship type. Further inquiry revealed that John's Arikara mother had told him to regard Mary as a sister. Mrs. Bear Leg wants her son to take all of his relationships

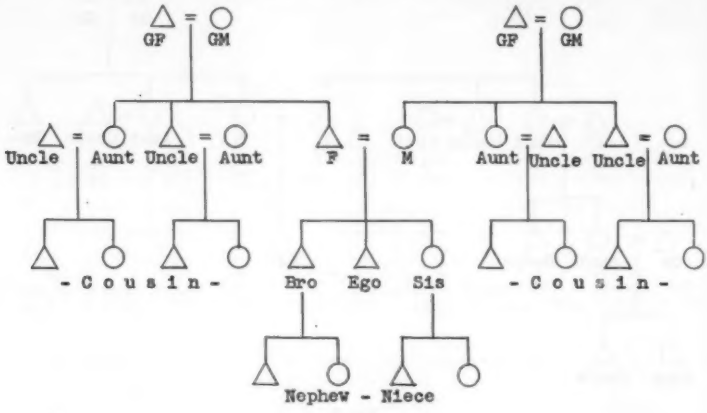


FIG. 3

in the Arikara way, and she attempts to teach him Arikara kinship. I checked with Mary, who informed me, "I know it's not right, but he calls me sister so I call him brother." The use of sibling terms for cross-cousins is one stage in Spoehr's series in the breakdown of a pure Crow system. But in this case it is a borrowed change introduced by an Arikara and not one that arose from the situation in Lone Hill.

John's total kinship pattern indicates that he follows Hidatsa usage with most of his father's relatives and Arikara usage with all of his mother's relatives, who reside in another village at Fort Berthold. As his mother seems to be the dominant member of the family in these matters, John also refers to some of his father's relatives according to the Arikara way. I pointed out this apparent confusion to John, who replied, "I still don't know about these relationships yet, except what they say to me. The old folks each tell me their way. I'm catching on slow but sure." The net result of this situation is that while John knows how to interact with each specific relative he does not have a consistent total kinship system. John, now 24 years old, understands neither

the Hidatsa, Arikara, nor the white kinship pattern. His case illustrates the confusion found among the children of mixed marriages in which the parents have not established a clear-cut identification with any one social system.

To summarize thus far, the two kinship patterns found in Lone Hill are: (1) the traditional Crow terminological system, which remains the basic type for the village and has not changed, at least since 1862 when Morgan first gathered Mandan and Hidatsa kinship terms; and (2) a white generational system employed primarily by the acculturated families. There is also a separate set of English terms used in the Crow pattern for the benefit of outsiders and an inconsistent terminological pattern in some Indian homes in which a member of an alien tribe has come to Lone Hill through marriage.

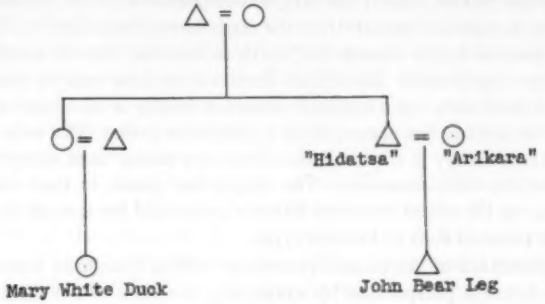


FIG. 4

TWO PROCESSES

These data, considered in conjunction with the findings of Spoehr and of the Schmitts, indicate that there are two distinct processes involved in kinship change. The first process consists of the slow modification of a kinship system as evidenced by an orderly and progressive shift in type. This process is best illustrated by Spoehr's complete series in the transition from a pure Crow to an American generation type. Although the shift described by Spoehr was an indirect result of acculturative factors, the slow modification of a social structure may be due to internal forces that are not necessarily associated with culture contact (Eggan 1941; Gough 1952). The second process is a more radical change evidenced by an abrupt jump from one type of kinship system to another. In the second case, reported upon in this paper, the Indian and the white systems exist simultaneously without intermediate variations in kinship patterns.

Both processes of change, slow modification and sudden transformation, occur together in Lone Hill—and they also take place together among the Southeastern tribes. Spoehr emphasized the progressive stages in the breakdown of a Crow lineage system but he also recognized the more rapid type of kinship change (1947:198). The Mandan-Hidatsa seem to differ from the Southeastern tribes in terms of the relative importance of each process.

The first process of slow modification has not affected the Crow kinship terminology and associated behavior in Lone Hill, but it has changed other aspects of social life in the village. There are many changes in content. A man recently modified the strict mother-in-law respect relation, not by the aboriginal practice of giving his mother-in-law a scalp, but by presenting her with an imitation war bonnet purchased from a mail order house. Innumerable other examples could be cited in which known changes in cultural content have not precipitated changes in the established social structure (cf. Gough 1952:85). An extreme illustration of this is provided by the previously mentioned case of English kinship terminology being used in the Crow kinship pattern. There are, however, many structural changes: the destruction of the native ceremonial life has decreased the religious importance of the father's lineage, the moieties are almost inoperative, the clans have diminished in importance, there is a general laxity among the youth in meeting certain kinship obligations, village organization has shifted from a close-knit unit to one of widely separated households, the matrilocal extended family is no longer a residence unit, and the lineage has ceased to be a corporate group. The only change in the Crow terminology is that vocative terms are rarely used except on public and community-wide occasions. The important point is that the gradual modification of the social structure has not proceeded far enough in Lone Hill to cause an internal shift in kinship type.

The significance of the second process of radical change in Lone Hill may be seen in broader perspective by examining the kind of acculturation now taking place among the Mandan-Hidatsa (Bruner 1953). In every acculturated family that uses the American generational system there has been an Indian-white intermarriage within three ascending generations. Individuals in these families make an abrupt and almost total conversion to white ways, while the rest of the people in the village continue to live by more traditional Indian standards. In the families in which there has been a mixed marriage, the entire white system comes across in the socialization process. Among the Southeastern tribes the shifts in kinship terminology were the result of adaptive responses to changing conditions (Spoehr 1947:225). The presence of the white generational system in Lone Hill is due to neither cultural "borrowing," adult learning, nor rearrangements in the social structure precipitated by new conditions. Individuals in Lone Hill learn the American kinship system in the English language within the intimate domestic family situation. Although no single factor is ever sufficient to explain a highly complex acculturation situation, it would seem that these differences in the mechanisms of change may account for the differences in kinship patterns found among the Southeastern tribes and the Mandan-Hidatsa. (In a future paper, I shall document and describe the process of individual conversion to white ways in greater detail.)

The converted Indian embraces a new way of life and comes to internalize a set of goals and values which are incongruent with the Crow lineage system. The kinship ties and obligations of the Crow system incorporate and reaffirm

the older societal values. Kinship provides the means for mutual aid and co-operation in the village as well as a mechanism of social control for ridiculing and cajoling the nonconformist. While the Mandan-Hidatsa social system, with its wide range, embodies the primary societal values of sharing, generosity, group co-operation, and village unity, the converted Indian shifts to the more individualistic American values of accumulating goods and wealth as ends in themselves. Any member of Lone Hill who interacts with others in terms of the Crow kinship system and who meets the obligations of that system will remain an "Indian."

The majority of the Indians who have converted to white ways tend to leave the village to seek their fortunes elsewhere (cf. Spicer 1940; Tax 1941). For this reason, there were few cases of American type kinship systems found in Lone Hill—most such people had already left their village. If all the individuals who adopted a new kinship system as part of a more inclusive conversion experience were to remain in Lone Hill, they might well produce radical changes in the existing social structure of the community. Their departure is a major factor making for the retention of traditional Indian ways. However, some of the converted Indians do continue to live in Lone Hill and do become sources of change in the Crow kinship system.

Individuals in Lone Hill who do not fulfil their kinship obligations destroy the solidarity of the group and the unity of the system. The converted Indian will avoid any kinship tie which involves his sharing material wealth or giving of his time or possessions. One man has developed devious means to accomplish this. He deposits all of his money in a bank so that when people ask him for a loan he can honestly reply that he does not have any money around. He avoids any situation in which he might be called upon to make donations, and, as such, he never attends the social "give-away" dances. This man has consciously narrowed the range of his relationships but he has done so selectively. He will not address another by a kinship term if there is any possibility that he may have to help that person. On the other hand, he does not avoid his joking relatives, for through the structured joking he can make critical remarks about Indian ways which would not be permitted in any other context.

SITUATIONAL DETERMINANTS

As converted Indians who use the generational system do interact with their many relatives in the village who use the traditional lineage system, some mechanism of adjustment is necessary. Two relatives who follow different kinship systems may have different conceptions of the proper way of relating to one another. The adjustment is usually made through one of the persons' adopting the other's system for that *particular* relationship. Every acculturated individual in the village who ordinarily uses the generational system will use the Indian lineage system with *some* of his relatives. The converse also occurs, as unacculturated Indians sometimes use the American generational system. Individuals thus adjust to the alternate kinship systems, which tends to minimize the changes in the systems themselves.

Individual adjustment to alternate ways of structuring relationships is neither alien nor unique to the Mandan-Hidatsa (Firth 1930). Overlapping kinship ties tend to be characteristic of small societies with widely extended kinship systems. In addition to consanguineal and affinal relations, the Mandan-Hidatsa extended kinship to the clan, linked clan, moiety, ceremonial system and the supernatural world. As a result, almost every individual in the village was related to everyone else in more than one way. The closest relationship, as defined by the Mandan-Hidatsa, was usually taken in any given case, but factors unique to the individuals involved were also important (Bowers ms.). Today the people often discuss these dual ways of being related and they tend to regard the white way of reckoning kinship as one more way of being related, comparable to the alternate ways within the Indian system.

The following examples will illustrate how the adjustment takes place.

A young girl teased an unacculturated woman, after which the woman said to her, "You're supposed to be my *icawi* [father's sister] and you are not supposed to tease me, but that's O.K., we can take it the white way."

In referring to an acculturated girl, an older woman remarked, "She don't talk Indian. She is supposed to call me mother but she calls me auntie. I would be glad to hear it if she called me mother but she won't use it anymore. She's my daughter but she won't understand so I call her niece. All of that family acts like white kids."

An old woman of the village called a young converted Indian boy by the term "son." He realized that she could be either a mother or a father's sister to him in the Indian way but he did not know which. He came and asked me to find out from my genealogical charts because, as he said, "I can't figure it out and I want to act right with the old lady."

An acculturated woman refers to her mother's brother as an uncle and addresses him as uncle. He follows the traditional system, continuing to call her younger sister and considering himself as an older brother. Even though the terms are taken from two different systems, neither party seems to be especially disturbed. This is an unusual case and tends to occur only when there is no marked discrepancy in the external behavior.

Frank Bruce, who recently returned to the reservation, was related to William White Duck in two ways—their fathers were in the same clan, which required strong obligatory joking, and Frank was a "father" to William, which called for a father-son respect relationship. William had lived on the reservation all his life, understood both relationships, and quite properly, in terms of the traditional system, took the father-son blood relation rather than the clan tie. He talked politely to Frank and considered him a father, whom one never teased, although he never addressed him as father. Frank did not know that he was related to William either way and on one occasion teased him. William then said in public, "This old man of mine wants to take clans as the rule." Thereafter they teased each other on every possible occasion. All this was explained to Frank after it had taken place, as he had not understood that he had changed an existing relationship.

Two individuals were related to one another in a dual way: one a joking grandfather-grandchild relationship, and the other a respect father-son relation. These boys were roommates at boarding school, during which time they teased one another and acted more like "roommates" than in accordance with either of the two possible kinship relationships. One said of the Indian way, "We left it home." When they returned to the reservation they continued the teasing relationship which had arisen from the school situation. Eventually they came to use grandfather-grandchild kinship terms as a means of justifying to others their mild joking. In this case, it was fortunate that a kinship bond existed to validate their behavior on the reservation.

Schneider (1953:228) has suggested "that one important function of the use of kinship terms as forms of address is in imposing on the situation a kind of sanction which enforces conformity with the social definition of the relationship." In some situations the people of Lone Hill consciously manipulate kinship terms to serve their personal ends. Interaction between the acculturated and unacculturated is not unstructured or loosely defined, but it does have an element of ambiguity as there are two relatively clear-cut alternate systems. A few individuals take advantage of this in rather subtle ways.

At a political meeting, a converted Indian, who ordinarily uses the generational system, addressed a prominent member of the opposing faction as a "father." He used the Crow system in this situation knowing full well that his political rival would not be so indiscreet as to criticize a "son" in public.

An unacculturated woman of the village came into the house of a wealthy converted Indian and addressed him as "older brother." She sat down and indicated that she had some important business in town but had no means of transportation. The converted Indian, as he was preparing to drive her to town, whispered to me, "The only time she ever calls me 'brother' is when she wants something."

CONCLUSION

In earlier times both the Mandan-Hidatsa and the Southeastern tribes had a pure Crow type lineage system. After acculturative influence an American generational type kinship system is found among the two groups. In the Southeast the orderly and progressive shift in kinship type was characterized as a process of slow modification of the existing system due to adaptive responses to changing conditions. In Lone Hill, the lack of intermediate variations in kinship patterns suggests a rapid process of change caused by early socialization in those families in which there has been white intermarriage.

The comparison of static types and categories at different times will yield only partial information on the processes of change and persistence. If we are satisfied simply with answers to the question of *what* change has taken place, it may be sufficient to study the beginning and the end of a cultural sequence. But only through examination of an ongoing sequence and of the actual contexts in which a choice of alternatives is made can we say *how* the change has happened and thereby gain insight into the process itself. And the deepest understanding of social change may best be achieved by direct study of the actual mechanisms and processes involved.

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Totemism in a Changing Society[1]

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BECAUSE it has rarely been possible to carry out studies of Australian aboriginal tribes over any considerable time-depth, analysis of processes of change has generally been neglected in the literature. This limitation has often been reinforced by a reluctance to examine primitive society from a historicist point of view, a reluctance often rationalized by calling such a viewpoint "conjectural history"—a pejorative label which often covers legitimate inference and known historical fact as well as "conjectural history."

This paper attempts to view the totemic system of one Australian tribe, the WaṇiNdiljaugwa, in time-depth. The material itself is particularly susceptible to such analysis since historical sequence may be clearly discerned, enabling us to see how totemism, usually conceived of in terms of a fixed order of the Universe laid down in the Creation Period, in fact changes as society changes.

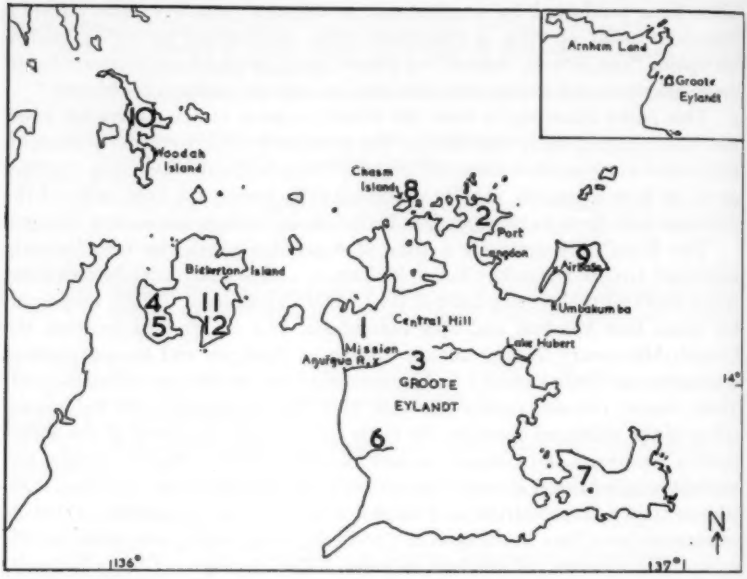
The WaṇiNdiljaugwa are a tribe of Australian aborigines who formerly inhabited Groote Eylandt, Chasm, Bickerton, and Woodah islands, and other minor islets off the western coast of the Gulf of Carpentaria. Today, they number some four hundred and fifty individuals, and are divided between the Church Missionary Society mission station at Aŋuŕgwa and an independent settlement at Umbakumba (Ambugwamba), both on Groote Eylandt itself. Their closest cultural connections are with the Nuŋgubuju and Balamumu tribes of the mainland opposite, the latter tribe being a subgroup of the northeastern Arnhem Land congerie named the Murngin by Warner (1937), but nowadays called the Wulamba (Berndt 1951). With these tribes, the WaṇiNdiljaugwa traded, intermarried, and fought, while some of the mainland religious ceremonies have been introduced to Groote Eylandt, undergoing some modification in the process (Mountford, in press).[2] The language of Groote Eylandt, EnIndiljaugwa, is structurally almost identical with Nuŋgubuju, being a prefixing tongue with multiple classification of nouns, but differs radically from Nuŋgubuju in vocabulary (Capell 1941).

The first white contact with the island occurred as long ago as 1623 (Heeres 1899:6), but paradoxically enough, this part of Australia was not brought under direct administrative control until the twenties of this century. Casual contacts with whites during the intervening period did occur, however. Flinders landed on Groote Eylandt, although no natives were contacted (Flinders 1814:183–98); around the turn of the century, various whites touched at the island, since there was increased activity in this region as a result of the building of the Overland Telegraph Line and the development of the pastoral industry.

The mission was originally started in 1921 for halfcastes from the mainland, but later the missionaries turned their attention to the WaṇiNdiljaugwa themselves. In 1938, Little Lagoon in Port Langdon in the northeast of the

island became an emergency landing place for flying boats on the United Kingdom–Australia route, and in the same year the Umbakumba settlement was started. During the war, the Royal Australian Air Force established themselves on the island. It is not surprising, therefore, that in a short time, processes of disintegration of the indigenous culture are already well advanced.

There has been lengthy contact with other nonaboriginal peoples, however. Makassans from the Celebes used to visit the coast of Arnhem Land annually from at least the last quarter of the eighteenth century until 1907, fishing for



MAP I. Tribal Territory of the WaṇiNdiljaugwa. *Key:* Numbers indicate clan territories (see Table 1).

trepang, pearl shell, pearls, etc., while turtle shell and other commodities were also collected. This led to considerable modification of the hunting-and-collecting pattern of aboriginal life, since the WaṇiNdiljaugwa were employed by the Makassans as divers, etc., while turtle shell, pearls, and other commodities were stored by the aborigines to trade with the Makassans when they arrived with the northwest monsoon.

The seasons in this part of the Gulf are marked by this northwest monsoonal wind—the hot, wet season—and the southeast trade wind of the cool, dry season. The north wind blows irregularly for a short time between the two seasons, but the two only are recognized, unlike the six seasons recognized by aborigines on Cape York Peninsula and the Arnhem Land mainland (Thomson 1939, 1948/9). Specht has stressed the marked changes in the en-

vironment which occur throughout the year, and the extent to which aboriginal food-gathering and hunting activities alter according to seasonal variations in animal and plant life (Specht, in press). He further stresses the essentially coastal character of the economy of the Arnhem Land aborigines. Both these features considerably influence the social life, and in particular the totemic system, of the WaṇiNdiljaugwa.

The tribal name, WaṇiNdiljaugwa, is that of the largest clan, probably the oldest stratum of the population, pushed into the southeast corner of the island. There are two moieties, with six clans in each moiety, an equal arrangement of clans which, as will be seen later, has no special significance. Although the moiety plays the key role in social life, and is the exogamous unit, the moieties are unnamed, but a relative term, with the root *-gabura*, is employed to indicate moiety affiliation. One can thus indicate "we-moiety-fellows" and "they-moiety-fellows." Ego will refer to *jirenigabura:* we-moiety-fellows, while the man he is talking to will use the identical word to refer to himself and his moiety-fellows in the *opposite* moiety. The root *-gabura* may be used with a wide range of prefixes, indicating other categories of moiety relationship, e.g., *dadagabura:* she-moiety-fellow, *girenigabura:* you-all-moiety-fellows, etc.

The clans have no name other than that of the clan members themselves, often derived from a clan-territory name (e.g., the WaṇuṇaDjaragba, from Djaragba, in the north central area of Groote). There is, moreover, no word for clan or moiety as a social category. The clan is a patrilineal group which possesses certain totems in common, although it may share some of these with other clans, and has a particular clan territory to which, however, it was by no means confined in its economic and social activities. The clans with their totems are given Table 1.

THE TOTEMIC SYSTEM

The totemic system of the WaṇiNdiljaugwa has many features of the classical Australian type, as described, for example, for the Aranda by Spencer and Gillen (1927), but it exhibits certain characteristics that are far removed from the classical type of totemism and which help to throw some light on the development of totemism in general.

On Groote Eylandt, there is no ritual attitude toward the totem; if an animal, it may be killed and eaten by anyone; increase ceremonies are not performed, although remains of this ritual can be discerned, and it has probably died out quite recently. Neither is there any concept of the totem as an eponymous ancestor, while today the tradition of a Creation Time is not clearly recognized by the WaṇiNdiljaugwa, although it is implicit in myth and ceremonial.

The principal totems fall into three groups: totems which are either features of the topography of the islands or natural species; the wind totems; and the ship totem. Three clans of Moiety 1 share the same principal totemic complex, while four clans of Moiety 2 share the ship totem.

TABLE 1

CLANS AND TOTEMS

*Clan**	*Totem* (principal totems first)
	MOIETY 1
1. WaŋuŋaDaṟbalaŋwa	Northwest wind; cypress pine; parrot fish sp. (*jembiṟgwa* and *jinuŋwenuŋbuŋa*); dugong; echidna; scrub fowl; goose; crow; paperbark tree sp.; *jiwunduruŋa* fish
2. WaŋuŋaDjaragba	North wind; cicatrice; *guŋguŋa* fish; *jinuŋbuŋa* snake; bluetongued lizard; "small stones"; seagull sp. (*idjara*)
3. WuraGwaugwa	Central Hill; stingray; sawfish; combfish; shark ray; Jinuma (Aŋuṟgwa) rivers; cottonwood tree; parrot; Neribuwa and Dumariŋenduma
4. WaŋuŋAmadada	Central Hill; stingray; sawfish; combfish; shark ray; Jinuma (Aŋuṟgwa) rivers; cottonwood tree; parrot; Neribuwa and Dumariŋenduma
5. WurEŋgiljaŋba	Central Hill; stingray; sawfish; combfish; shark ray; Jinuma (Aŋuṟgwa) rivers; cottonwood tree; parrot; Neribuwa and Dumariŋenduma
6. WaŋuŋAwerigba	Sea eagle; garfish; grasshopper
	MOIETY 2
7. WaŋiNdiljaugwa	Southeast wind; bark canoe; casuarina tree; stringybark tree; stringybark; "place"; caterpillar; barbed spear; praying mantis
8. WaŋuŋAmulaŋwa	Ship; shark; porpoise; conch shell; bailer shell
9. WaŋuŋAmagadjiragba	Ship; shark; Catalina complex; dove; turtle
10. WaŋuŋAŋwurerigba	Ship
11. WuraMaṟba	Ship; coconut
12. WuraMura	Snake; Maliṟba Hill; water; waterfall; frog; hawk

* See Map I for location of clan territories.

Under the first heading fall the totemic complexes deriving from myths in which Central Hill, and Maliṟba Hill on Bickerton, feature. Here we have not merely a single totem but a complex of totems, all of which represent important elements in a myth. Thus, the myth concerning Central Hill relates how Central Hill (*Jaŋdaṟŋa*) first came from the mainland, stopping at various spots, each of which he found unsatisfactory, until he finally reached his present location. Linked to this myth is another concerning Parrot, who came over at the same time, and again another myth concerning two mythical individuals named Neribuwa and Dumariŋenduma and their activities in hunting, killing, and cooking Stingray. Finally, attached to this group of myths is the story of Shark Ray, Stingray, and Sawfish, who opened up the present Aŋuṟgwa River and Lake Hubert on the east coast. We thus have several myths linked in the aboriginal mind, and possibly part of some epic story at one time, al-

though there are indications of the coalescence of myths. Maliṟba Hill on Bickerton Island, on the other hand, has no origin myth, but is important in the myth of the journey to the Land of the Dead, Bralgu, and is associated with Snake (cf. the Rainbow Serpent), water, waterfall, and water creatures such as frogs, water python, etc. As we shall see later, there are other totems besides these principal ones.

The only principal totem of the conventional Australian type, concerned with only one natural species or object, is that of the WaṇuŋAwerigba clan, Sea Eagle. There is a myth connected with this totem which explains how man came to use fire in cooking fish, but the Eagle came to eat raw fish. All these mythical creatures are represented by various natural features, usually rocks, with Central Hill, the highest point in the Gulf, as the most striking. It may be noted here that the principal rivers on Groote find their source in the hilly interior, and the relationship in mythology between Central Hill and the Jinuma River and Lake Hubert is an expression of this fact. Lake Hubert, when viewed from a distance, strongly resembles a stingray in shape, so that the significance of Stingray in the myths is not far to seek, nor can it be put down merely to the economic importance of this animal as a source of food.

Turning to the wind totems, we find an absence of origin myths.[3] Since these totems are possibly of Makassan origin, this absence would be to some extent understandable. There are traditional places associated with the winds, however, or rather traditional directions, the obvious ones, since it appears that ceremonies for the manufacture of winds were performed at various places. In the case of the all-important northwest and southeast winds, the ceremonies were not necessarily held on Groote itself, but on the mainland to the northwest in the first case, and often on small islets off the southeast corner for the southeast wind. The north wind, a less important one, is not connected with any special spot, but an unidentified island is sometimes mentioned (possibly Funnel Island in the Eirene Archipelago, where natural holes in the rock produced noises when the wind blew over them; cf. Wilkins (1928:231). Clearly, the wind totems are connected with the annual visits of the Makassans: the very names of the winds indicate this, being of Malay or Makassarese origin, though the indigenous terms still exist in some cases. Thus, the southeast wind, *mamariga*, is also called *duŋgwara* (Standard Malay *tĕnggara;* Makassarese *toeñggâra*); the northwest wind, *bara*, is derived from the Malay *barat* (Makassarese *bârâ*); *dimburu*, the north wind, presents a problem in that *timborô* in Makassarese means "south." The south wind (not a totem) is *dalada* (Standard Malay *sĕlatan;* Makassarese *sallâtañg*). Rose's hypothesis that the symbols for the wind used in paintings were derived from the shapes of Makassar sails in various winds is a further indication of the Makassar origin of the wind totem (1947), while Searcy notes of the Makassans that "when leaving a camp-place, some ceremony was gone through for the purpose of making wind" (1909:81). The clans differ greatly in size, the WaṇuŋAmulaŋwa of Chasm Island now having only one living male member, whereas the

WaṇiNdiljaugwa and the WaṇuṇaDaṟbalaŋwa together form over one third of the total population. It is worthy of note that these last two clans, one in each moiety, possess the key wind totems.

In the case of the ship totem, its recent origin is clear enough. Ship is associated with a locality in southern Bickerton Island and with a myth explaining not only how the Makassans made ships there, but also how humankind came to be divided into black and white, an arrangement for which the Makassans, logically enough, were responsible. There is no ceremonial activity connected with Ship.

The distinction between "principal" and "secondary" totems is one drawn by myself, and should not be taken too rigidly. Nevertheless, a native of the appropriate clan will nearly always give one of the wind totems, Ship or Eagle, when asked for his totem; these are clearly regarded as more important than the other totems. Those totems usually given first, and discussed more than any others, are treated here as principal totems. One may, however, be given any one of a number of totems, and in the case of the Central Hill–Stingray complex and the WuraMaṟba, no one totem receives a primary stress. Where several clans have the same totem, it is usually the clan in whose territory the totem is finally said to have stopped his wanderings, and where it is to be found as a rock or other feature, that is regarded as more closely associated with the totem than any other clan.

In order to appreciate how the totemic system can change it is necessary to consider the secondary totems, of which there are dozens, some being almost lost and others being in the process of becoming accepted. As with the primary totems, the secondary ones may be either single totems or complexes of totems derived from myths. In the case of the single totems, these have generally been borrowed from the mainlanders. Most secondary totems have myths associated with them, similar in nature to the myths already cited; such are the myth of the Porpoise and the Shark; of Hawk; the totemic song in which figure Stringybark Tree, Stringybark, Bark Canoe, Casuarina Tree, Caterpillar, Barbed Spears, etc., and the myth of a Shark who jumped out of the water at various spots round Little Lagoon commemorated by trees, finally disappearing into a billabong at Augwalamadja near Umbakumba, where he is now visible as a patch of reeds in the center of the billabong.

On considering this compendious list of totems, one might form the conclusion that this is an attempt to fit various features of the environment into the scheme of the two moieties. The apparent compendiousness of the list, however, is in fact a product of historical accretion rather than any attempt to systematize the natural environment according to some philosophic scheme. We have already indicated how mainland totems are added to the Groote system. There are strong indications that aesthetic inspiration also plays a large part as a mechanism by which new totems rise to social prominence. Thus totemic songs about two ships, the "Cora" and the "Wanderer," are the inventions of one man whose principal totem is Ship, an extremely imaginative individual with a great flair for the dramatic and absorbing story. He has also

composed songs about the Catalina flying boats which used to land in his clan territory, and which are in part water-borne transport like his principal totem, Ship. Further, the Airbase itself has provided him with another linked subject since the troops stationed there gave rise to the song "Army."

It can thus be seen that Groote Eylandt totemism has undergone considerable modification; whereas it is often difficult to show any historical sequence in primitive societies, the material here shows a definite succession of totemic features, thus revealing that totemism is by no means a fixed and unchanging system. Actual historical events can give rise to new totems through the creation of songs and myths. In support of this observation are certain linguistic usages. The word *alauwudawara* means, basically, a story or myth; it is also used to describe string figures, bark- and other paintings, and any object, especially small objects, of an intriguing or attractive nature—I have thus heard it used to describe an attractive mole on a person's body, a very small and neatly contrived medical phial, and so on. Since there is no word for totem, one asks, "What is your myth (*alauwudawara*)?" or "What is your song (*emeba*)?" The aesthetic connotations of these linguistic usages are obvious: it is the myth and song which now constitute the principal external features of a totem, and any invented song is liable to be assimilated to the totemic system.

As far as the content of the totemic myths is concerned, it can be seen that the majority of totems relates to the natural order-animal species, topographical features, etc. Thus we find an emphasis on the seasons, as symbolized by the wind totems (with the additional modern connotation of the important Makassan trading expeditions); sea products and the major features of the landscape, especially those well-watered areas which provide richer animal and plant resources and a perennial water supply; and a large number of animal species, not necessarily edible. There is no justification, however, for reading into the totemic system a thoroughgoing systematization of the natural order. Much of the superficially comprehensive "systematization" is merely cumulative, while many vital features of the environment are omitted—including most plants and nearly all the hundred and more fish species known to the aborigines. Certain important animals such as dingo and wallaby have been borrowed from the mainland where they are totems. Even in tribes where a more rigid schematism has been noted, it is generally limited in scope and by no means covers the whole natural order (cf. Kelly 1935, *passim*).

As well as symbolizing man's concern with the natural order, the totems clearly serve as symbols of group affiliation. At the level of the clan, the principal totem or totemic complex is often insufficient to distinguish one clan from another: Clans 3, 4, and 5, for example, possess the same principal totemic complex. It is here that the secondary totems have a diacritical significance. Thus the WaṇuŋAmulaŋwa, besides possessing the principal totem, Ship, also have Conch and Bailer Shells, Porpoise and Shark; the WaṇuŋAmagadjiragba have the totems derived from the Jumping Shark myth together with the Catalina-Army group and certain single totems borrowed from the mainland;

the WaṇuŋAŋwurerigba, an intercalary group, have various Balamumu totems, while the WuraMaṙba also have Coconut. The fact that these four clans possess Ship in common is undoubtedly a reflection of their mainland origin, as revealed in genealogies. The group of clans sharing the Central Hill–Stingray complex has no distinguishing secondary totems, but this fact may be understood from the recent formation of two clans, the tiny WurEŋgiljaŋba clan and the large WaṇuŋAmadada. Amadada is the mainland opposite Bickerton Island, and these people are all descendants of a recent Nuŋgubuju immigrant of philoprogenitive tendencies who fitted himself into the Groote system when he married several wives there, by adopting the totems of the Wura-Gwaugwa clan of Groote itself. The WaṇuŋAmadada clan therefore has only these totems, but Nuŋgubuju totems are also occasionally referred to and may become secondary totems in time. It is noticeable that all the movements of totemic creatures in the myths parallel contemporary canoe- and foot-routes, and thus give an indication of immigration routes along which movements have taken place in the past. There are variations in these routes, however. Most easterners and the Balamumu regard the mythological movements as starting from Balamumu territory, but those WaṇiNdiljaugwa closer to the Nuŋgubuju describe the movements as starting from Nuŋgubuju territory. Such discrepancies, then, may be seen to be socially conditioned; the myth varies according to the social background of the teller.

A factor making for complication is the sharing of totems by all other clans in the same moiety. This means that the number of totems which a person can regard as his own is considerably extended, and reflects the importance of the moiety as the key social unit of social organization. In ceremonial, the moiety plays the chief part, and moiety-fellows "help" to sing totemic songs of all clans in their moiety, while moiety designs are painted on initiates' chests. One may blow the drone pipe (*jiraga*) for members of the opposite moiety, and dance to their totemic songs, but one may not sing their songs. In this way, the social solidarity of a moiety is symbolized in the sharing of totemic songs, and the interdependence of the moieties is expressed in the *jiraga*-playing and dancing, as in the simultaneous singing of moiety songs by the two groups during a ceremony. Finally, the totem serves as a symbol of group allegiance at the level of the tribe. In a superficially pointless piece of buffoonery at the conclusion of major ceremonies, a representative of each totemic group participates in the buffoonery; members of both moieties thus co-operate in the performance. By virtue of correlations between Groote moieties and those of the Balamumu and Nuŋgubuju, social relations with those tribes are also carried on within a definite framework. Thus the Balamumu Dua moiety is equivalent to the WaṇiNdiljaugwa Moiety 2 and the Nuŋgubuju Mandaju; the Balamumu Jiridja is equivalent to the WaṇiNdiljaugwa Moiety 1 and the Nuŋgubuju Mandiridja.

Totemism on Groote Eylandt can thus be regarded as having two major aspects: It expresses the relations between Man and Nature, and it also expresses the relations between man and man. Radcliffe-Brown has already

made valuable corrections to Durkheim's solidarist approach, where Durkheim tends to limit the significance of totemism to its function as a "concrete representative or emblem of a social group" (Radcliffe-Brown 1952:125), sacred because of the powerful sentiments of solidarity arising from group membership (Durkheim 1937:294, 315, *passim*).

In correcting Durkheim's approach, Radcliffe-Brown has shown that animals and plants are used as totems not merely because they are symbols of social groups. He notes that the relationship of the aborigines to their environment is a special one, conditioned by their dependence "on the hunting of wild animals and collection of wild plants" (1952:126). Moreover, it is a relationship to a particular environment, not to Nature in the abstract: the location of plants and animals, water supplies, etc., for example, is intimately known to the aborigine; his environment therefore has a selective interest for him. This selective or limited interest is further conditioned by his way of life—the natural order around him is not a *tabula rasa* waiting to have some form of classification inscribed on it, and the aborigine creates his totemic categories in accordance with his primary interests, interests which arise from his hunting-and-collecting way of life and not from contemplation of "Nature" in the abstract. The existence of uranium or bauxite deposits would be a matter of indifference to the tribal aborigine (or the latter would interest him only as paint); what does concern him is animal and plant resources.

We have noted, however, that many of the totems in the compendium of the WaṇiNdiljaugwa are not natural species. One might therefore question the validity of Radcliffe-Brown's stress on the importance of hunting and collecting as the essence of the aboriginal relationship to Nature expressed in totemism. One might go further and inquire whether totems such as sleep, diarrhea, and dysentery (Radcliffe-Brown 1929:399 ff.) or vomiting sickness (Curr 1886:296–303) are natural phenomena of quite the same order as the natural-species type. By using such methods, one might attack the whole concept of totemism as a social category, but it would be falling into the facile empiricism that Goldenweiser used to "destroy" totemism on the Pacific Northwest coast of America (1933) to adopt such a methodological approach. Although the totemism of the WaṇiNdiljaugwa reveals certain differences from the classical type of Australian totemism, their common features are clearly recognizable. Because there are differences, we need not conclude that the phenomenon does not exist; rather are we dealing with different phases in the development of totemism.

Nor should we conclude, because so much stress is laid on myth and song, and because these are the external features of the totem today, that totemism is merely an aesthetic phenomenon. The social connotations of totemic organization have already been described, but, more than this, the songs and myths themselves are merely vehicles of expression by means of which new important social events are given social recognition. No doubt many of the songs which merely concern ephemeral changes or isolated incidents would pass out of existence, but songs referring to historical events which led to important

changes in social relations, such as the arrival of the Makassans and the whites, are likely to persist as part of the totemic system. The development of new totems, therefore, is not solely a matter of aesthetics, but is determined by the importance of the social material out of which new totems are created. No sharp distinction, therefore, is drawn by the natives between these "historical" songs and traditional totemic songs.

Classical totemism stems from a special relationship to Nature, conditioned by a hunting-and-collecting existence and a particular form of social organization. Just as for most aboriginal tribes, this basic pattern of life was maintained virtually unchanged over the centuries, so the order of the Universe as expressed in totemism was fixed and unchanging—the order laid down in the Creation Period. For the WaṇiNdiljaugwa, this relatively timeless pattern of life was shattered by the arrival of the Makassans, and the totemic system was modified accordingly. Finally, their new dependence on the white man's goods, and the impact of the war, caused further modifications of the totemic system. The timelessness of totemism has given way to an ever changing system, just as life itself is changing. The Creation Period is not spoken of today, nor consciously visualized as such. It is merely said that the activities of the totemic creatures took place "a long time ago." A people with a history, the WaṇiNdiljaugwa think of the period of these activities in historical rather than mystical terms, as pre-Makassan rather than of the Creation Period, although they recognize that this earliest period was a period of "miraculous" events.

As there is little ritual significance attached to the totems today, it is mainly their role in relation to social groups that makes the totems still of some importance. If these groups disappear, as a result of settlement life, it is likely that the totems will be forgotten, and that new songs, which might normally have been assimilated into the totemic system, will merely become a genre of songs commenting on events of social interest, songs which will be purely secular and unconnected with any corporate groups.

NOTES

[1] This paper is based on one read to Section F of the Australian and New Zealand Association for the Advancement of Science at its Canberra meeting, January, 1954. My field work in the Northern Territory was carried out under the auspices of the Australian National University. The orthography used for EnIndiljaugwa is in accordance with the system developed for Australian languages, based on that of the International Phonetic Association (Capell 1945); the orthography for Malay and Makassarese is taken from Hendershot and Shellabear (1945) and Matthes (1859) respectively. The WaṇiNdiljaugwa have previously been referred to in the literature as the Ingura (cf. Tindale 1925 and 1926).

[2] The ceremony described by Mountford as the *Arawaltja* is the principal religious ceremony on Groote. Its correct name is *Amunduwuraria; arawaltja* probably refers to the *auwurawalja:* shades, huts, i.e., the huts constructed for the ceremony.

[3] The existence of wind totems is by no means limited to Groote-Eylandt. They are found also on the mainland opposite, while the Euahlayi had several wind totems (Parker 1905:16–19, 82ff.) Nowhere, however, are they so important in the whole totemic system as on Groote Eylandt.

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Letters to the Editor

On La Barre's Review of *The World's Rim*

Sir:

I am motivated to write you because of the singularly devastating review of H. B. Alexander's *The World's Rim*, by Weston La Barre, which was published in the February number of the Anthropologist (57 [1955]:182). It seems to me that the work has positive merits which the reviewer has failed to recognize.

Alexander's work is intended to demonstrate that there is a philosophy of nature and of man implicit in the myths and rituals of the culture of the American Indian and that notwithstanding differences in symbolic expression the underlying ideas bear close similarities to Greek and Oriental thought. Alexander is concerned to indicate not only the obvious similarities of theme which a comparative study of myth and ritual disclose but the symbolic character of these rites and their philosophical significance. As Kluckhohn indicated in his Foreword to this work, Alexander's work is a contribution to the appreciation of the universals, the common understandings, implicit in historic human cultures and expressed in a dramatic mode. In brief, it is through his sympathetic understanding of the common humanity of the Indians as expressed through their dramatic symbolism that the author makes his most significant contribution. This is essentially a work of philosophical interpretation of ethnological material which the author studied at first hand over a period of many years. The idea of the near-universal symbolic significance of Indian art and ritual is one which psychologists since Jung have come to recognize and which folklorists concerned with comparative and distributional studies, such as Thompson and Barbeau, have long since indicated. Surely at this late date it should no longer be necessary to argue on behalf of the symbolic character of myth and the philosophical dimension of all human cultures. If anthropology is to be truly a science of man, the anthropologist must be concerned with cultural universals as well as empirical differences, and primitive myth and ritual offer rich material for the analysis of such universals. Of course, the material must be used critically, but comparative studies must not be rejected on principle simply because ethnologists of a previous generation have abused them, or have assumed psychological principles which are now outmoded.

David Bidney, *Indiana University*

Reply: Elementargedanken Noch Einmal?

It is not a question of denying the symbolic character of myth or culture; it is a question of the nature of symbolism. A symbol is never *discovered* in nature; it is *invented* by man. Therefore, apparently similar symbols cannot have been discovered because man everywhere was attending to the same inevitable world. If we find two societies with the same symbol, how, then, shall we explain this? Accidental unrelated parallels? Historical diffusion? *Elementargedanken?* I should say that apparent similarities in the Hako and Greek fauns dancing are accidental, unrelated, and parallel only in Alexander's mind; I take it that historical diffusion is improbable in this case.

If there are *Elementargedanken*, their existence will not be established (to use Spier's forceful phrase) by the Frazerian method of "daisy-picking." If we "explain" an Old Testament story in terms of an Australian myth "parallel," or the Pawnee in terms of

the classical Greeks, this requires our having established the existence of *Elementargedanken* beforehand: we need to establish the very assumption under which we are operating. And whatever the "psychological unity of mankind" may mean, it does not mean that the same symbol-*content* will inevitably be ground out everywhere by the same-model human mind. Everything we have learned from comparative linguistics, for one thing, would controvert this supposition.

As for Jung, the "archetypes" inherited in the "folk unconscious" will seem more plausible to me when we have established the Lamarckian inheritance of acquired physical characteristics first; until then, possibly the simple social inheritance of symbols (through enculturation or diffusion) will more economically account for the observed phenomena. On this score, I think Kroeber, in 1918 (review of [Collected Papers in] *Analytical Psychology* and *The Psychology of the Unconscious*, by C. G. Jung [AMERICAN ANTHROPOLOGIST 20: 323–24], p. 324), quite properly scouted

> the assumption, apparently typical of the [Jungian] school, that the symbols into which the "libido" converts itself are phylogenetically transmitted and appear socially. The machinery of this assumed process is not examined. Its reality is considered established by the adduction of examples which may be so interpreted. Now if the [Jungians] are right, nearly all ethnology and culture history are waste of effort, except in so far as they contribute new raw materials. If, on the other hand, current anthropological methods and the psychobiological assumptions underlying them are correct, the phylogenetic theories of Jung and his collaborators are only a mistaken excrescence on their sounder work. Mutual understanding will not progress as long as the two tendencies go their conflicting ways in ignorance of each other.

It is evident that the philosopher Alexander never absorbed these anthropological experiences and understandings. It is precisely because there is *not* any universal Jungian dreambook and symbol-dictionary and panhuman meaning-encyclopedia that we have to do field work, ethnographic and linguistic. I have devastated no universal symbols; there were never any real ones there to begin with. Meanwhile, anyone on the lookout for cultural universals would better start out, in my opinion, working pretty close to that concrete cross-cultural phenomenon, the human body—and not with the most arbitrary and most contingent products of men's minds, their symbols.

WESTON LA BARRE, *Duke University*

ON CHRISTIANSEN'S REVIEW OF *Studies in Cheremis Folklore*

Sir:

Professor Christiansen's gratifying and interesting review of our Cheremis book in the AMERICAN ANTHROPOLOGIST (56 [1954]:925) contains four errors of fact, omission, or interpretation, which I would like to call to your attention.

1. Contrary to the statement in the heading, the book in question was not published by the Indiana University Press, but constitutes No. 6 of the Indiana University Publications, Folklore Series, which is entirely independent of the press.

2. The work is Vol. 1 of a series of several volumes to follow, as is indicated on the cover of the book. Vol. 2 will appear shortly as No. 22 of the Viking Fund Publications in Anthropology, and Vol. 3 is in active preparation.

3. The Cheremis are not located in Central Russia as stated in the first paragraph; by implication, this would place them in Soviet Central Asia, which is not the case. The Cheremis are actually located in East Central *European* Russia.

4. The population estimate of half a million is attached, in the first paragraph,

mystifyingly, to the date of 1830, which is incomprehensible in the context. The date given in the book is 1941.

THOMAS A. SEBEOK, *Indiana University*

[The Review Editor must take full responsibility for the errors mentioned in items 1 and 2 of Sebeok's letter. All headings are given their final editing in the office of the Review Editor. With respect to the third paragraph, Dr. Christiansen informs us that in Norway and other countries of western Europe the term "Russia" is ordinarily used to apply to European Russia. Dr. Christiansen also remarks that the population estimate for the Cheremis for 1830 is derived from the Finnish Encyclopedia and was intended to give some historical perspective on the size of the group.—D. F. A.]

MENOMINI RESEARCH

Sir:

It has been called to my attention that my monograph, *Sociocultural and Psychological Processes in Menomini Acculturation*, recently published as Volume 5, Culture and Society Series, University of California, is conspicuously lacking in references to work on the Menomini, published or unpublished, by Dr. Sidney Slotkin. I would like to take this means of clarifying the situation and of making amends for one important omission.

I refer in three places to Dr. Slotkin: once in the Acknowledgments, once in the appendix, and once in a footnote on page 244. Although these references mention only his study of the Menomini language, and Dream Dance songs that he tape-recorded, I did not mean to imply that this was the extent of his work with the Menomini. His "Menomini Peyotism" (*Transactions of the American Philosophical Society* 42 [1952]) and "Social Psychiatry of a Menomini Community" (*Journal of Abnormal Social Psychology* 48 [1953]) are well known, and he will soon have published an extensive and important study of the Menomini Dream Dance, in addition to other articles.

The bulk of my monograph was written by the Spring of 1952—as my doctoral dissertation—and revised for publication during the Winter and Spring of 1953. The revision included one new chapter on "Psychological Configurations and Psychological Process," approximately twelve additional pages on the Dream Dance, War Dance, secular music, and pan-Indianism, and scattered additions throughout the text. I had already published "Experimental Design" (*Southwestern Journal of Anthropology* [Spring, 1952]) and an article on Peyotism (*Psychiatry* [May, 1952]).

None of the publications, the original dissertation, or the revisions described above in any way utilized or were influenced by Dr. Slotkin's publications or manuscripts, with one exception that I wish to make amends for now. This is the forthcoming publication on the Dream Dance by Dr. Slotkin. I saw this in manuscript before I wrote four pages in the revision, added to what I had already put in the dissertation on the Dream Dance. I used materials from my own field notes, collected over four summers of field work and resulting from participant observation in about twenty-five Dream Dance ceremonials. But my use of these materials was influenced by Dr. Slotkin's much more complete treatment. As I retrospectively consider this, and after examining my field notes, I believe this applies specifically to the exact allocation of some of the offices in the ceremonial organization, described on page 68 (bottom paragraph); to the phrase (page 69), " . . . but there is no counterpart for the warriors, 'bigstick', 'drumbeater', messenger, and pipe man" (among the women); and to the statement on page 70 (third

paragraph from the top) " . . . the specific deities and spirits connected with the ritual, the system of the universe symbolized by ceremonial objects and behaviors, are all identifiably Menomini in character . . . [to end of sentence]." I would not have made these statements with the same degree of certainty if I had not seen Dr. Slotkin's manuscript. If my very summary description of the Dream Dance corresponds otherwise to aspects of his much more full and detailed analysis, it is because we used the same informants from a native-oriented enclave of less than thirty active adults.

I gratefully acknowledge the influence described above. I cannot explain this striking omission (except possibly in psychoanalytic terms). I intended no deception, and I am indeed sorry that this omission occurred in my citations and that this manuscript was not added to the bibliography already completed much earlier (no items were added). Otherwise, his work, and my own, stand entirely on their own. They are not, in fact, particularly competitive, since he deals with facets and dimensions that I only touch upon, and vice versa. I have not, however, ever considered my work with the Menomini only a "Rorschach study," so there is inevitable overlap. Readers may see for themselves where we differ, and where we agree, in those areas of overlap. I believe that this is a desirable condition that we could hope for in other situations where we have the interpretations of only a single person.

GEORGE D. SPINDLER, *Stanford University*

CORRECTION

Sir:

In my article, "The Lappish Herding Leader: A Structural Analysis" (AMERICAN ANTHROPOLOGIST 56 [1954]:1076–80), the title of the chart on page 1079 has been omitted. It should read "The Changing Composition of Band C." On the same chart the figure "3" on the band composition for 1931 should be placed under the symbol for the husband of Person No. 4 rather than under the symbol for the wife of Person No. 2.

ROBERT N. PEHRSON, *Sibi, Baluchistan, Pakistan*

This Issue and Others

(Continued from page ii)

According to Wissler, Kroeber, Herskovits, and other American anthropologists, culture develops and spreads independently of human society. Separate cultural elements, and even whole complexes of cultural elements, can travel by themselves, enter other cultures, or, as American ethnographers put it, "diffuse." This idealistic position borders on the semi-mystical conceptions of Frobenius, who considered culture to be a living being, that it was not the product of human activity and that man was only the passive bearer of culture, the object of its action, etc.

Thus, the followers of the theory of "cultural areas" studied culture as something outside time and space, while the separate manifestations of material, spiritual, and social life of the society they gave out as special, unchanging characteristics.

In the article these particular criticisms are not explicitly tied to politics. On the other hand, the author argues that acculturation studies purport to show that there is no difference between the cultural ties of the various peoples in the past and imperialistic colonization which cut short the independent development of various

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Book Reviews

Edited by DAVID F. ABERLE and RICHARD K. BEARDSLEY

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peoples of the world. An ethnographer investigating the process of "acculturation" can study the life of the population in the Reserves and note the fact of change in occupation, referring this to the influence of European or American culture. But he is not obliged to discover the internal reasons for these changes.

In this way "acculturation" becomes a law of social development, which justifies the destruction of cultures, the destruction of populations of Africa, America, Australia, Oceania in the period of colonization, and the monstrous oppression and racial discriminations of the present time.

The implication that American anthropologists in any way justify oppression and racial discrimination is absurd to those who know us at all. The whole spirit of American anthropology is strongly to the contrary. One cries out for a chance to talk to Sobchenko and his colleagues, to come to know them and to have them understand our work. In the new friendly atmosphere, perhaps an exchange of anthropologists between the U.S.A. and the U.S.S.R. would be especially fruitful.

GENERAL AND THEORETICAL

The Institutions of Primitive Society: A Series of Broadcast Talks. E. E. EVANS-PRITCHARD and others. Oxford: Basil Blackwell, 1954. viii, 107 pp. 7*s* 6*d*

This little book contains eight essays originally given as talks over the famed Third Programme of the B.B.C. Their purpose, as set forth in the Introduction, was "to show laymen what kind of contribution can be, and is being made by anthropological studies of primitive societies to various branches of knowledge. . . " (p. v). The Introduction also states that "the contributors have tried to state what is held by most of their fraternity . . . " (p. vi). It remains for the reader to decide whether or not the contributors have succeeded in this undertaking.

The first essay is by Evans-Pritchard, on "Religion," a concept he does not, however, attempt to define. In this essay the author traces very briefly the history of anthropological generalizations concerning "religious" phenomena, noting three more or less different approaches: rationalistic speculations about origins, characterized by the writings of Tylor, W. Robertson Smith, and Max Müller; "psychological" explanations in terms of feelings of awe (Marett), or of anxiety (Malinowski), or of projection (Freud); and sociological explanations (Durkheim and his colleagues of the *Année Sociologique*). It will surely surprise no reader to learn that Evans-Pritchard favors the third approach. Moreover, he insists that generalizations about "religion" be based upon functionally oriented field studies of particular societies, followed by comparative studies having at first very modest objectives: "One must not ask 'What is religion?' but what are the main features of, let us say, the religion of one Melanesian people; then one must seek to compare the religion of that people with the religions of several other Melanesian peoples who are nearest to the first in their cultures and social institutions; and then after a laborious comparative study of all Melanesian peoples, one may be able to say something general about Melanesian religions as a whole. One can only take this long road. There is no short cut" (p. 9). Sound advice, from an anthropologist who has written probably the best ethnographic account of African religious institutions.

While the essay on "Religion" concentrates on methodology, Firth's chapter on "Orientations in Economic Life" deals descriptively with economic phenomena in primitive societies. First of all, he is careful to point out that *all* societies called "primitive" (" . . . little use of metals; no complex mechanical apparatus; no indigenous system of writing . . . " [p. 15]) do have the essentials of an "economy," i.e., rationally based actions involving "a selection of ends, a choice of means to fulfill them, an estimation in terms of scarcities, comparative qualities, alternatives, a concern for margins of input and output, a reckoning of future requirements, and a foregoing of some present satisfactions" (p. 15). In these respects primitive societies do not differ in kind from technologically more complex ones, but—continuing Firth's exposition—they do differ in degree. For example, some primitive societies do not possess monetary systems of any sort, or merely the rudiments of one; others have dual systems—one for the market and one for gift exchanges. Again, he notes that in general members of primitive societies "make their economic relationships do social work" to a greater extent than occurs in other societies. In a methodological aside Firth also touches on a point brilliantly elaborated in his *Elements of Social Organization* concerning the generalization of the concept of transaction to include "social" factors as well as factors narrowly defined as "economic"—adding a judgment, heartily endorsed by this reviewer, that all such actions are susceptible to economic analysis.

Leach's chapter on "Aesthetics" is a rather chatty critique of European critics' views about the plastic arts of primitive peoples. For a layman it undoubtedly succeeds in clearing up some of the usual misconceptions about the subject. However, the anthropological specialist will probably consider several of Leach's generalizations too sweeping, even when addressed to a general audience (e.g., "Primitive peoples do not have church buildings that need to be decorated" [p. 35]). And this reviewer hopes that Leach will publish additional documentation for his intriguing hypothesis that there are "characteristic differences of artistic expression which go along with differences of ethical ideal" (p. 37), in support of which he cites correspondences in cultures of North west Coast Indians, New Zealand Maoris, and Victorian England.

The chapter on "Law" by Peristiany is a *tour de force* in terms of substance and presentation. Accepting Aristotle's observation that there are as many types of law as there are kinds of social bonds, he assigns himself the task of describing how sectional interests in a stateless society come to be transcended. Rather than attempt to deal with all "primitive society" in a few pages, he utilizes his own field notes on the Kipsigis (Kenya) as representatives of "at least one major class of primitive society" (p. 40), and proceeds to describe with remarkable comprehensiveness and clarity the cultural context and actual operation of their legal processes. This compact essay may have proved tough going for many of its radio listeners, but to this reviewer the inferences to be drawn from it have far wider application than the author's caution permits him to assert.

In his chapter on "Political Institutions" Gluckman assigns himself a similar task ("how the 'more primitive' societies, those without governmental institutions, live in political unity, under peace and good order," p. 66), but he generalizes more explicitly, and concerns himself primarily with processes of group expansion, segmentation, and elaboration of countervailing social ties—all in terms of their significance for maintenance of social order. In his view, conventions of exogamy and problems of technology require the expansion of social (and political) ties beyond the face-to-face group, and this expansion has been accomplished by means of widening networks of ties based on kinship, trade alliances, and common membership in sodalities and in congregations. Meanwhile, he notes a tendency for social units to segment, and then become bound together again by additional networks of cross-cutting alliances. To exemplify some of these processes Gluckman cites the Zulu, and then ends this excellent though informal résumé with an analysis of the present state of Negro African political institutions.

Fortes begins his essay on "Mind" with this statement: "Whatever other and more exalted meanings the concept of mind may have, to me as an anthropologist it makes sense only as a shorthand term for certain aspects of the activities of living people" (pp. 81–82). With this, he sets out to discuss certain respects in which "mental activities" of primitive peoples differ from or resemble those of Western peoples. The main differences dealt with in this highly readable and persuasive little essay concern concepts of time and space. In the matter of language, he attributes to it a universal role as instrumental skill and medium of social relations—charged, also universally, with moral authority and emotional values. And what applies to language applies also to other aspects of culture: that is, among no peoples can a hard and fast line be drawn between "the inner realm of mind and the outer world of society and custom" (p. 84). However, primitive societies do differ from the more complex Western ones in being more homogeneous, with the result that "the fusion of the private world of the individual and the public world of the community is more obvious" (p. 84). For another resemblance Fortes points to the more exotic traits of primitive cultures (witch doctors, etc.) which,

far from being distinctively "primitive," are to be found everywhere—although perhaps less commonly and publicly in Western societies. Fortes also takes issue with the guilt–shame dichotomy of cultures, holding that a sense of guilt may be no less feeble in non-European societies than in European ones; it is merely that the primitive societies having so-called "shame" cultures usually possess better means of dealing with guilt, and with other forms of psychological stress, so that the individual is not left bearing the burden alone.

Entirely different is Lienhardt's approach to the study of the mentalities of primitives in his chapter on "Modes of Thought." In it he endeavors to explain some of the problems faced by an anthropologist in comprehending and describing how members of a primitive community "think." It is not possible to do justice to this skillfully reasoned and lucidly presented essay in a short paragraph, and anthropologists and linguists are urged to read it in its entirety.

The one chapter remaining to be discussed, "The Family and Kinship," by Layard, forms a curious intrusion into this collection of otherwise up-to-date essays. In this chapter, and side by side with some acceptable and widely recognized generalities (e.g., the universal importance of kinship for social cohesion; the recognition by every society of both matrilineal and patrilineal principles), are many picturesque generalizations of this order: "Among predominantly matrilineal communities religion tends to become extravert (a masculine function) and to be expressed in a mass of complicated ritual" (p. 59). Moreover, the major theme of this essay is an anachronistically phrased reconstruction of the evolution of societies. Social systems undoubtedly "evolve," but not necessarily along the teleologically oriented routes laid out by Layard.

It would make interesting and probably sharply contrasted listening to compare what top-ranking American anthropologists would say if given the topical assignments treated in these broadcasts!

DOUGLAS L. OLIVER, *Harvard University*

Essays in Sociological Theory. Revised edition. TALCOTT PARSONS. Glencoe, Illinois: The Free Press, 1954. 459 pp. $6.00.

Unfortunately, there appear to be only two views concerning the work of Parsons. There are the disciples, for whom all problems are exegetical or, at most, involve slight elaboration of "The Frame of Reference"; and there are the scoffers, for whom Parsons is merely a latter-day scholastic with an unintelligible writing style. Although this reviewer has never had the honor of meeting Parsons, it seems almost certain, judging from his published views concerning the proper development of science, that the last thing he wishes to become is the center of a cult, inspiring blind devotion and irrational rejection. Such, however, he unfortunately seems to have become, so that with each new publication, signaling one or more new "break-throughs" (as Parsons calls ideas), the saved are raised to new heights while the damned are faced with several hundreds more pages which they probably will never read and hence are confirmed in their damnation. Since the books *do* become progressively more unintelligible if one has not kept up and read them in order of appearance, the gulf between the saved and the damned becomes ever wider. It is therefore most useful to have a new edition of these *Essays*, providing a bridge over which those few who remain uncommitted may walk from the earlier *Structure of Social Action* to the more recent *Toward a General Theory of Action*, *The Social System*, and *Working Papers*.

Eight papers have been added in this new edition; four are concerned with particular empirical phenomena and four with considerations of general theory. Of the first

group, three ("Democracy and Social Structure in Pre-Nazi Germany," "Some Sociological Aspects of the Fascist Movements," and "Population and the Social Structure of Japan") are concerned with problems arising out of World War II and its aftermath. Readers familiar with the earlier edition will find little that is unfamiliar here. These papers are in the tradition of Parsons' wartime interest in social structure in the large —on the level of the nation-state. They are accompanied here by the excellent series on European and American social systems which appeared in the earlier edition. Also, there is one newly written (1952) paper which properly belongs with these, "A Sociologist Looks at the Legal Profession." Taken together, this group of papers may be said to represent Parsons' bringing up to date of Max Weber's earlier grand synthesis of the Western institutional system, though the separate essay on Weber, which formed the preface to Parsons' translation of *The Theory of Social and Economic Organization* and was included in the earlier *Essays* does not appear here.

Parsons brings Weber up to date in two important respects: on the one hand, he takes account of trends within the Western institutional complex which have emerged with greater clarity since Weber wrote, and he makes use of the vast amount of empirical research which has gone on since that time. One example, of particular interest to anthropologists, will suffice. In "The Kinship System of Contemporary United States," he explicitly takes as his point of departure the type of analysis of kinship terminology and behavior which has been so fruitfully applied to nonliterate societies by anthropologists (unlike the earlier edition, incidentally, this one contains the diagram illustrating the structural analysis). In one important respect, Parsons improves upon usual anthropological practice. He analyzes not only the direct structuring of behavior around consanguineal and affinal relatedness but also the functions of kinship which arise from the fact that persons who play kinship roles also play roles of other kinds. Thus he points out that, though the modern Western occupational system is specifically oriented *away* from direct kinship structuring, still the "open, multilineal, conjugal" type of system has important functions for the occupational system in that (1) it prevents particularistic kinship ties from interfering with universalistic criteria for recruitment and performance, and (2) it provides a "dynamo" for occupational achievement in the form of the isolated, mobility-oriented nuclear family.

Parsons also brings Weber up to date in another respect: to Weber's macroscopic institutional analysis he adds a much more sophisticated treatment of personality systems in relation to social structure. This broadening of analytical attack is the fruit of Parsons' great interest in and familiarity with the work of Freud and the modern students of personality. Thus, in the new paper on the legal profession, he is able to analyze not only the gross structural social-control functions of the lawyer as part private professional practitioner and part officer of the state but also his semipsychiatric social-control functions in face-to-face interaction with his client.

This brings us to the other major dimension of Parsons' work and to the other new additions to the *Essays*. Taken as a whole, beginning with the *Structure of Social Action*, Parsons' professional career has been devoted to the development of nothing less than a new conceptual synthesis for social science. "Synthesis" is the important word here for, contrary to the view of some of his critics, his aim is *not* the creation of a "Parsonian School" of sociology (pp. 351–52). His aim is a pulling-together of the work of his predecessors and contemporaries, and his neologisms are in large part the result of the attempt to label uniformly and to bring into conceptual interrelation ideas which have drifted apart as social science has become more complex and more compartmentalized. Parsons' earlier efforts in this direction (represented in this volume by "The Role of

Ideas in Social Action" and "The Theoretical Development of the Sociology of Religion") were concerned with bringing together and restating the modifications in the earlier "utilitarian" theories of social action which a developing sociology had shown to be necessary.

Now Parsons, together with his colleagues at Harvard and elsewhere, has undertaken to wed sociological theory to the personality theory which has grown out of the work of Freud. Two new papers ("Psychoanalysis and the Social Structure" and "The Prospects of Sociological Theory") reflect this new commitment. The idea of such a marriage is of course not new. Anthropologists and sociologists have long been concerned with socialization as a process through which institutionalized values are "built into" personalities. However, perhaps partly because of the simple role structures of the societies with which most anthropologists have worked, socialization has most often been seen as a relatively simple chicken-and-egg process. Society and/or culture are presented to the developing individual, he absorbs them, and he becomes an adult with the motivations appropriate to the society and/or culture. If I may paraphrase a rather complex argument, Parsons' new view of the personality-culture-society relationship is this: personalities, cultures, and societies are systems which are partially interrelated and partially independently variable. They are interrelated in that each arises from the basic situation of actors-in-interaction. They are independent in that each has its own systemic character—its own kind of integration among constituent elements. Parsons and his colleagues believe that they are now on the track of a conceptual synthesis which will take account of both the independent variability and the interrelatedness of these systems. Thus each of what they call "pattern variables" of institutional role definition has a counterpart in a type of "object orientation" in personality systems and a type of "value orientation" in cultural systems. A concrete element of action will have meaning on each of these levels, but its meaning on one level will not be the same as, or determinant of, its meaning on other levels (pp. 360–63). Parsons' reasoning is far too complex to be described adequately in a few lines, but it is this reviewer's opinion that his handling of the personality-society-culture problem is far and away the most adequate yet suggested.

Two other papers, new in this edition, may be noted more briefly. "Social Classes and Class Conflict in the Light of Recent Sociological Theory" is a critique of Marx written for the centenary of the *Communist Manifesto*. The final paper, "A Revised Analytical Approach to the Theory of Social Stratification," which has appeared also in Bendix and Lipset's *Reader in Social Stratification*, is a detailed working out, in one area, of the theoretical developments noted above.

These papers, in brief, provide samples of nearly every phase of Parsons' widely ranging and deeply probing thought. They are often ponderously written, but social scientists will ignore them at great intellectual peril.

LLOYD A. FALLERS, *East African Institute of Social Research (Kampala, Uganda)*

Bibliografia Crítica da Etnologia Brasiliera. HERBERT BALDUS. ("Commisão do IV Centenário da Cidade de São Paulo, Serviço de Comemorações Culturais.") São Paulo, 1954. 859 pp., 11 plates.

This is a monumental and erudite piece of work. It is not the usual classified bibliography, but rather a guide to the literature of Brazilian Indian ethnology from 1500 to 1953 which may be used both by the inexperienced student and by the specialist. Baldus, a German-trained ethnologist who studied under Thurnwald, has been a dedicated student of the Brazilian Indian for almost thirty years. This volume is the fruit of his research in the libraries of South America, the United States, and Europe, and

the intimate knowledge of Brazilian Indian cultures gained from his field research is apparent throughout. The bibliography includes 1,785 published items on Brazilian aboriginal cultures. These sources range from the sixteenth-century classical accounts of the Tupinambá on the Brazilian coast to travel accounts of nineteenth-century naturalists, more recent scientific studies by German, French, North American, and Brazilian ethnologists, and obscure and brief articles by casual observers. Baldus has provided us with a critical comment on each item listed. Some of these comments are but brief warnings as to the veracity or soundness of the author's observations, or short statements about the particular interest or value of a publication. For other items, Baldus has written full-length critical reviews. These reviews and commentaries, along with his historical and interpretative introduction to the volume, amount to a history of the ethnology of the Brazilian Indian. Quite wisely, Baldus has included many important publications on Tropical Forest and Marginal tribes from beyond the national frontiers of Brazil. Furthermore, the volume is an efficient research tool. Each item is numbered. There is a subject index which is not very complete. There are also, however, a geographical index, a tribal index, and an index of authors. Baldus has more than achieved his aim, which is "to prevent my colleagues and students from spending time, money, and intellectual effort in doing what I have had to do—that is, spend years and years in libraries and spoil my sight reading countless books, many of which contain nothing which merits note. Naturally, I also desire to clear up uncertainties and to provoke new doubts, as well as to prevent the 'discovery' of tribes unknown to discoverers, but very well known in the literature" (p. 40).

Of course, the publication of this critical bibliography is most important to students of South American Marginal or Tropical Forest ethnology. But it should be of interest to all anthropologists for the picture it gives of modern Brazilian ethnology. Outside the English-speaking world, Brazil is undoubtedly one of the major centers of anthropological research; in Latin America, only Mexico has shown a comparable development in this field of study. But because Portuguese is not widely read outside Brazil (although it is spoken by over fifty million people in the Americas alone), few of the younger and highly productive ethnologists of Brazil are well known abroad. As Baldus states in his introduction, "Brazilian ethnology, a field of study cultivated in the past almost entirely by Europeans and especially by Germans, has now become the domain of the native born of this country." So rather than discuss the early chroniclers, the classical accounts by naturalists, or the modern scientific studies published in German, French, or English—all of which Baldus has evaluated for us—I shall limit my discussion to publications by Brazilian ethnologists which have appeared in Portuguese in recent years. These publications regularly appear in the *Revista do Museu Paulista*, in *Sociologia*, in the *Revista Municipal*, in the *Revista de Antropologia* (all published in São Paulo), and in the *Boletim do Museu Nacional* of Rio de Janeiro—as well as in book-length monographs and theoretical books.

The most experienced and most productive student of the Brazilian Indian is Baldus himself, as indicated by the numerous articles and books which he has published from 1924 to the present. His critical evaluation of his own contribution is much too modest. Although German by birth and training, Baldus has identified himself closely with Brazilian anthropology, and most of his scientific publications are written in Portuguese for Brazilian journals. Furthermore, through his teaching at the Escola Livre de Sociologia e Política in São Paulo, he has made a fundamental contribution through his many students. Among them are some of the most active younger men in the field of Brazilian Indian studies.

Outstanding among the younger Brazilian anthropologists is Florestan Fernandes.

In a series of book-length studies, he has made use of a functional approach to analyze the data furnished by historical documents on sixteenth-century Tupinambá society. Fernandes writes with "audacity and penetration," to use Baldus' words. His *Organização Social dos Tupinambá* (Social Organization of the Tupinambá) and *A Função Social da Guerra na Sociedade Tupinambá* (The Function of War in Tupinambá Society) show not only a careful use of historical data but also an awareness and understanding of contemporary European and American ethnological and sociological theory. When added to Métraux's studies of Tupinambá material culture and religious life, Fernandes' works provide us with a remarkably complete picture of this extinct aboriginal culture of the Brazilian coast.

Another Brazilian anthropologist whose work deserves wider notice is Egon Schaden, who is Professor of Anthropology at the Faculty of Philosophy, Science and Letters of the University of São Paulo. To date his major contributions have been in the field of mythology. His *Ensaio Etno-sociológico sôbre a Mitologia Heróica de Algumas Tribos Indígenas do Brasil* (Essay on the Mythology of Several Indigenous Tribes of Brazil) is, in Baldus' opinion, "one of the most important works on modern Brazilian ethnology." In recent years, Schaden has carried out field work among the remnants of the Guaraní tribes of southern Brazil.

Baldus also calls attention to the work of Darcy Ribeiro and Eduardo Galvão, both of whom are at present high-ranking staff members of the *Serviço de Proteção aos Indios* (Brazil's National Indian Service). Both of these young anthropologists are already well known in Brazil as field workers. Ribeiro is best known for his publications on Kadiueu religion, mythology, and art, which, to quote Baldus again, "show remarkable ability both in field reporting and in 'armchair analysis' (*trabalho de gabinete*)." Since 1949, Ribeiro has carried out several expeditions to the Tupí-speaking Urubú tribe in northeastern Brazil; the results of this research are as yet unpublished, though he reports on them briefly in an article concerned with the scientific program of the Indian Service (item 1307 of the bibliography). Galvão has carried out field research among the Tapirapé, the Tenetehara, the Cayúa, and the tribes of the Upper Xingú. At present he is doing field studies on the process of Indian acculturation on the Rio Negro in the northeastern Amazon. The most important recent publication by Galvão, based upon research undertaken when he was a member of the Museu Nacional staff, *Cultura e Sistema de Parentesco das Tribos do Alto Rio Xingú* (Culture and Kinship System of the Upper Xingú River Tribes—*Boletim do Museu Nacional, Antropologia*, No. 14 [Rio de Janeiro, 1953]) appeared while Baldus' bibliography was in press and is only briefly mentioned (p. 264).

Among other highly competent contemporary Brazilian anthropologists Baldus comments upon the work in physical anthropology of Pedro Lima of the Museu Nacional; on the research among the Terena of Fernando Altenfelder Silva, who teaches at the University of Paraná; on the work among the Umutina and Krahó of Harald Schultz of the Museu Paulista; as well as on Gioconda Mussolini's interpretative studies based on the data collected among the Vapidiana (Wapishana) by the Benedictine missionary Mauro Wirth. Since Baldus' bibliography is limited to studies of Brazilian Indians, it does not include the work of people such as Thales de Azevedo, Antonio Candido de Melo e Souza, Octavio da Costa Eduardo, Lucilla Hermann, Ezra Borges Teixeira, Levy Cruz, Oracy Nogueira, René Ribeiro, and others who work in the field of modern community studies and on Afroamerican subjects. And it must be said that nowadays, perhaps, more Brazilian anthropologists are interested in contemporary

Brazilian culture than in aboriginal groups. Yet Baldus' bibilography gives us evidence of the highly active group of anthropologists now at work in Brazil.

A perusal of this bibliography also discloses several facts about the Brazilian anthropological group. First, there is little recent work in archeology, physical anthropology, and linguistics; an overwhelming majority of the younger group are social or cultural anthropologists, whether they work with indigenous or with modern communities. Second, it is clear, at least among the younger group, that the ethnologists broadly share a functional-historical point of view. Not one of them, except perhaps Eduardo Galvão, seems to have been influenced by the neo-evolutionary point of view of Leslie White and Julian Steward. Instead, they seem to have been influenced by people such as Benedict, Herskovits (in Afroamerican studies), Linton (*The Study of Man* has been translated into Portuguese), Redfield, Malinowski, Kroeber, Radcliffe-Brown (who taught for a short period in Brazil), and to a certain extent by the modern British social anthropologists and American sociologists such as Merton and Parsons. Except perhaps for René Ribeiro, whose publications are on Afro-Brazilian studies, I know of no Brazilian anthropologist whose primary field of interest is personality and culture. Thus, on the one hand, Brazilian anthropologists have a common ground in the similarity of their interests (Brazilian sociology is to a large extent synonymous with social anthropology); on the other hand, the "Brazilian School" is a limited development weak in archeology, in linguistics, in the modern genetic approach to physical anthropology, and in many seminal approaches to modern cultural and social anthropology. But the tempo of modern Brazil is rapid development and change, so that most anthropologists are being called upon to tackle practical problems—to help formulate realistic policies for the Indians, to study rural and urban communities for educational and economic development programs, or to participate in projects for the study of race relations for UNESCO. Anthropology has developed in Brazil just because it is closely allied to the national interests of the country.

Despite this preoccupation with research more directly related to current national problems, there is inevitably also a "last-hour spirit" (to use Baldus' term) among students of the Brazilian Indian. There is a feeling of the urgent necessity of collecting and recording data on the numerous primitive cultures threatened with extinction—a point of view comparable to that current in the United States earlier in this century regarding the necessity of studying the vanishing Indian cultures. As Baldus states, "We know the names of many tribes, but few have been studied. There are numerous authors and hundreds of publications that deal with our Indians, but few of these works satisfy modern scientific standards" (p. 39). Furthermore, several hitherto unknown tribes and formerly hostile ones have entered into peaceful relations with the officers of the Brazilian Indian Service within the last five years; these groups offer perhaps the last opportunity to study relatively unacculturated American Indian cultures. But time is short, for the truck, the airplane, the radio, and other apparatus of modern technology, plus the eagerness of Brazilians to take possession of the land, have increased the tempo and intensity of Western impact upon the remaining tribal groups of Brazil. Neither Baldus nor the reviewer advocates that Brazilian social anthropologists and foreign ethnologists interested in Brazil should turn entirely to gathering facts. Rather we wish to call attention to the Brazilian Indian as a field for theoretically oriented modern research. Baldus points out that there are several tribes such as the Krahó and the Carajá, for example, which would, like the Navaho and the Hopi, provide excellent laboratories for a wide variety of research problems.

Finally, it must be said that this important contribution to the study of the Brazilian Indian was published by the Commission for the IV Centennial Celebration of the Founding of the City of São Paulo and that the volume is dedicated to the XXXI International Congress of Americanists, which met in São Paulo in August of 1954. The city of São Paulo, therefore, must be congratulated for choosing to celebrate its four hundredth anniversary by publishing a series of important books on Brazil and by playing host to a long series of scientific congresses.

CHARLES WAGLEY, *Columbia University*

Die Anfänge des Eigentums bei den Naturvölkern und die Entwicklung des Privateigentums. W. NIPPOLD. The Hague: Mouton & Co., 1954. 94 pp. f 22.

The origin of the concepts of property and the development of different kinds of property in humanity are discussed from time to time mainly by economists, and are interpreted in political programs. Obviously, such comments remain pure speculation (cf. Jan Lt. Lewinski; *The Origin of Property and the Formation of the Village Community* [London, 1913]) as long as they do not take as their starting point the real facts which can be observed for some very old groups among the vast mass of primitives. It is the task of cultural anthropology to determine these tribes whose economic organization and social laws date back into an early stage of evolution of human culture as well as to investigate their ideas and rules concerning the forms of property.

Only about twenty years ago, for the first time, the famous scholar P. Wilhelm Schmidt (*Das Eigentum auf den Ältesten Stufen der Menscheit* [Münster i. W., 1937]) tried to analyze the whole bulk of the concepts, rules, and laws concerning the property of the so-called "Urvölker," the actual living tribes on the lowest cultural level, namely, the food-gathering stage; he derived from their practices the ideas and rules in that sphere of very early man. Nippold, now fortunately possessing better and more accurate observations and under more favorable circumstances, repeats the same attempt in his book but restricts himself to only three representatives of that very early group: the Pygmies in Central Africa, the Bushmen in Central Africa and the Negritos in Southeast Asia. Why he has made this restriction is not explained. But, in anticipation, my observation in this: in principle the same ideas and rules have been found among the Indians of Tierra del Fuego (cf. M. Gusinde, *Die Feuerländer* [Wien, 1931/37]), among the Indians of eastern Brazil (cf. A. Métraux in *Handbook of South American Indians* I:299), and among other tribes of that very primitive group. Following principles applied in the investigation of civilized people, Nippold works out to a historical certainty that the above mentioned nomads still follow the system of life in their material activity which essentially may have been that of earliest man, who can be described as a restless hunter and food-gatherer. It can hardly be questioned that very early mankind provided for its subsistence by food-gathering.

The three groups discussed here recognize two kinds of property: individual and common. Every person, regardless of age or sex, is able to acquire goods and prepare tools, and over these each individual has right of ownership and disposition. Barter is very little developed and occasionally presents are exchanged, but there is no individual ownership of the land or hunting area. Therefore private rights are in existence earlier than those of landownership, for women and children were recognized as full-entitled owners of property, with individual rights of disposition, equally as early as men. Obviously the total of owned objects on this low culture level adds up to very little in quantity, but the titles which legitimize the property rights are clearly defined. As far as communal or public goods are concerned, these primitives recognize only permissive

rights to certain districts of land. Each person is assigned to an individual local group, and every member is allowed to acquire whatever he needs within these limits. Since the whole social structure of the food-gatherers is the simplest structure, all rights and regulations in the sphere of property lack intricate and complicated details.

Are these well-analyzed facts helpful for the interpretation of the culture form specific for these food-gatherers as well as for the human race in general? No doubt about it. The essential structure of their life system is very old and has preserved some characteristic traits without the specializations and differentiations which are characteristic of more progressive culture types. Therefore what is typical in the culture of very primitive food-gatherers stays somewhat closer to the beginnings of the whole culture body of humanity. Obviously their cultural properties, in spite of great age, are not an accurate picture of the real beginning of all culture. Each of the actually living "Urvölker" has developed its own particular mode of life during hundreds of thousands of years and under manifold influences, and also in dependence on various environments—a statement which is overlooked by many critics.

Modern ethnology cannot and does not pretend to determine cultural facts by the help of an absolute chronology; it contents itself with a relative one. That means, in this case, that the culture of the very primitive food-gatherers represents an older type than agriculturists and cattle-breeders. Therefore the term "Urvölker," for which an equivalent term does not exist in other languages, means only the actual living representatives of the oldest culture types discoverable among the primitives of today.

So this fact stands: that the concept of property, and all the consequences involved in it for our very primitive tribes, have preserved some original traits from very early stages of human culture and, so far as they allow us to draw conclusions with historical certainty, show us that humanity did not begin with communistic organizations and regulations; that, on the contrary, individual personalities and individual rights were clearly developed close to the very beginning of human history. Evidently these factors correspond closely to human nature.

Nippold has presented the real facts in precise order without any prejudice and in analyzing them has avoided tendentious interpretations. It seems to me that this is the best recommendation which can be given to his work.

MARTIN GUSINDE, *Catholic University of America*

Mass Communications Seminar. HORTENSE POWDERMAKER (ed.). New York: Wenner-Gren Foundation for Anthropological Research, Inc., 1953. v, 151 pp.

Under the sponsorship of the Wenner-Gren Foundation for Anthropological Research, Inc., Powdermaker organized a meeting "at which the problems of mass communications, both theoretical and applied, were explored by forty-four scholars and administrators on an interdisciplinary level at a week-end seminar." This volume is a verbatim report of the four main presentations and the discussions that followed each of them. These included statements by Ralph Linton, Paul F. Lazersfeld, Harold D. Lasswell, and John Dodds. The general student of communications will continue to find the previous writings of the first three to contain more cogent statements of these authors' positions than could be stated under seminar circumstances.

Despite the presence of more than forty interdisciplinary social scientists, little of lasting importance emerged from the discussion. The reader's access to the verbatim record will, however, permit him to learn that the discussions were concluded in ample time to allow the serving of cocktails and that one member of the group had to absent himself in order to cook supper for his family. There was even the inevitable outburst

of indignation by a University of Indiana anthropology professor who decried the debasing effects of mass communications on the indigenous musical patterns of Indonesia. With refreshing candor, the Indonesian informant chose progress for his people in culture as well as in technology. For him, the development of new Indonesian music represented the attractive possibility of cultural pluralism, and a hallmark of Western value systems.

Powdermaker's brief summary of the seminar abounds with such platitudes as, "A continuing survey of images of ourselves and of self-images of the inhabitants in selected areas throughout the world is therefore essential."

The reviewer hoped for some clarification of the unique contributions and potentialities of the anthropological approach to the study of mass communications. The remarks of Morris E. Opler were particularly incisive. Opler indicated that in the past anthropologists have by and large been concerned with small and compact communities. As a result, their interest in communications processes has been limited. With the broadening of concern of some anthropologists toward large-scale societies and social change, the problems of understanding communications become more central for anthropology. Likewise, in his opinion, a concern for applying systematic and quantitative "sociological" techniques to anthropological field work becomes indispensable, and the professional training of anthropologists requires development in this direction.

MORRIS JANOWITZ, *University of Michigan*

ETHNOLOGY AND ETHNOGRAPHY

Wind River Shoshone Ethnogeography. D. B. SHIMKIN. ("Anthropological Records" 5, No. 4.) Berkeley: University of California Press, 1947. v, 42 pp., 2 plates. $.75.

Childhood and Development among the Wind River Shoshone. D. B. SHIMKIN ("Anthropological Records" 5, No. 5.) Berkeley: University of California Press, 1947. v, 37 pp. $.75.

The Wind River Shoshone Sun Dance. D. B. SHIMKIN. ("Anthropological Papers," No. 41.) Washington: Smithsonian Institution, Bureau of American Ethnology, 1953. 88 pp., 8 plates.

The above titles represent a partial development of the Eastern Shoshone material collected by Shimkin on the Wind River Reservation in 1937 and 1938. Shimkin has also published articles on Wind River Shoshone history and linguistics in periodical journals. These titles are as follows: "Wind River Shoshone Geography," AMERICAN ANTHROPOLOGIST 40:413–15 (1938); "Shoshone-Comanche Origins and Migrations," *Proceedings of the Sixth Pacific Science Congress* 4:17–25 (1941); "Dynamics of Recent Wind River Shoshone History," AMERICAN ANTHROPOLOGIST 44:451–62 (1942); "Wind River Shoshone Literary Forms," *Journal of the Washington Academy of Science* 37, No. 10:329–52 (1947); "Shoshone II: Morpheme List," *International Journal of American Linguistics* 15:203–12 (1949). While the latter material is not being reviewed here, its importance for our knowledge of the Wind River people should not be overlooked. Shimkin's work is of special significance in that, except for Lowie's early research, the Eastern Shoshone have been largely neglected by American ethnographers.

Childhood and Development among the Wind River Shoshone attempts, with felicitous results, the difficult task of interpreting "the general interplay of social and psychological factors" during the prereservation period. Shimkin first discusses formal and informal mechanisms by which the culture was transmitted to the young. There follows a description of the life-cycle and a chapter on "Processes of Socialization," which

constitutes a summary of the foregoing sections. He concludes the work with four life-histories. A fuller range of life-histories might have been desirable. Also, one could have wished for a fuller presentation of the results of the projective test which was given during the course of field work.

Curiously, Shimkin has made better use of Rorschach data in his most recent monograph, *The Wind River Shoshone Sun Dance.* The reviewer feels that the latter is by far the best product of Shimkin's field work. However, it should be stressed that only tentative conclusions are reached in the other two works covered by this review. He traces the origin of the Eastern Shoshone Sun Dance to the Plains Algonkians and presents also a reconstruction of its early history. The Kroeber-Spier hypothesis of the origin of the Sun Dance is accepted by Shimkin, but he points out several modifications upon its introduction into Shoshone culture. Lack of torture is attributed to Shoshone "restraint," and the fundamental "egalitarianism" of the Shoshone is cited to partially explain the absence of the participation of the military societies and of hereditary leadership in the Sun Dance. Egalitarianism, individualism, and restraint are also characteristic of the culture of the Great Basin Shoshone and call into question Shimkin's statement in *Wind River Shoshone Ethnogeography* that "Wind River Shoshone culture has been essentially that of the Plains for a good two hundred years."

The reviewer's own field data find considerable disagreement with the latter work. Shimkin has relied rather heavily on certain historical sources while failing to exploit the rich body of material found in government documents. He states that summer was a period of intertribal rendezvous in the Green River country. My informants, however, were unanimous in claiming that the entire summer was spent in small groups and that the purpose of the Fort Bridger visit was only trade and antelope hunting. It is difficult to see how a large number of people could have subsisted for any considerable period on the meager resources offered by the semidesert of southern Wyoming. Also, the present writer's field work showed only occasional penetration of the Powder River country. The seasonal penetration of Siouan hunting grounds by the weak Shoshone is questionable in any event. Finally, Shimkin's claim that "the horse did not at all improve the total sum of Wind River economy" is difficult to accept unless one takes "economy" to mean caloric intake. Even then, it is doubtful if the statement holds. In general, the work suffers from lack of a clear periodization in terms of contact with the whites.

ROBERT F. MURPHY, *University of Illinois*

Bishop Tamarón's Visitation of New Mexico, 1760. ELEANOR B. ADAMS (ed.). ("Publications in History," XV.) Albuquerque: Historical Society of New Mexico, 1954. 113 pp. $2.25.

In early New Mexico there was a dispute between the friars of the Franciscan Order and the secular clergy as to jurisdiction over the province, a dispute that flourished unabated from the founding of the colony to the end of the colonial period. During this time the Franciscans, under the authority of the Province of the Holy Evangel in Mexico, exercised control. They had originally accompanied Juan de Oñate to the province in 1598 and had helped to establish the first missions, in which they remained and provided such religious services as were allowed. As France V. Scholes says in his authoritative *Church and State in New Mexico,* "Ordinary ecclesiastical jurisdiction in all cases affecting friars, Indians, and Spanish colonists was exercised by the prelate of the Franciscans under authority granted by the bull *Exponi nobis* of Adrian VI, May 10, 1522, which gave the prelates of monastic orders the right to exercise quasi-episcopal power in areas where there were no bishops."

At the same time, the secular clergy, represented by the Bishop of Durango, claimed legal jurisdiction over New Mexico by authority granted in 1620 when this bishopric was created out of the old one of Guadalajara. Included in the new province was a vast, amorphous area that extended indefinitely northward. Lack of exact geographical knowledge of these lands and rivalry between the two branches of the Catholic Church led to the long conflict for power over New Mexico. According to the Bishop of Durango, New Mexico came under his jurisdiction by papal dispensation; but actually the prelate of the Franciscan Order in the province exercised full episcopal authority. On only three occasions did the bishop set foot in the distant region before 1800, and one of these was Bishop Tamarón's visitation of 1760.

The first of the bishops to inspect New Mexico was Benito Crespo, who became Bishop of Durango in 1723. In a visitation of his field in 1725 he went as far as El Paso, but was not able to continue up the Rio Grande at that time. In 1730, however, he examined the entire province and was thus the first one of his position to do so. Even though the Franciscans protested his claim, the Crown gave some support to his pretension to episcopal power.

The second bishop to demonstrate his authority over New Mexico was Martín de Elizacoechea, who visited the province in 1737, but the controversy over jurisdiction continued unabated. Such was the situation when Pedro Tamarón y Romeral became Bishop of Durango in 1758. Almost immediately he determined on a general visitation of his diocese, and he was engaged in this mission for nearly two years. His tour of New Mexico took place in the summer of 1760. With great diligence he inspected all the pueblos, except Zuñi, which he was unable to reach because his mules gave out in the intense summer heat!

Bishop Tamarón was a keen observer and kept a record of what he saw and heard in each of the pueblos and settlements. He maintained good relations with the Franciscans but otherwise paid them little heed. In his report he described each place, gave a census of its inhabitants, described the life and living conditions, and held confirmations. The Tamarón account of New Mexico thus gives a very good picture of conditions in the province, though it is limited almost wholly to the Pueblo Indians and Spanish settlers. It is a most important source of information on New Mexico in the eighteenth century.

Tamarón's recommendations for reform dealt chiefly with two matters: the superficial nature of Christianization of the Indians, owing to ignorance of the native languages by the priests, and poor defense against incursions of hostile Indians, especially the Comanches. Both of these problems had complicated New Mexico's existence for a very long time.

Adams' translation of Tamarón's report is clear and to the point, the historical introduction valuable, and the editorial notes informative. Basic material of this kind is giving us a better knowledge of New Mexico during the seventeenth and eighteenth centuries, periods for which we have all too little accurate information.

GEORGE P. HAMMOND, *The University of California (Berkeley)*

Directed Culture Change in Nayarit, Mexico: Analysis of a Pilot Project in Basic Education. GLEN FISHER. ("Middle American Research Institute," 17, preprint.) New Orleans: The Tulane University of Louisiana, 1953. 111 pp., 5 figs., 4 plates. No price listed.

Turrialba: Social Systems and the Introduction of Change. CHARLES P. LOOMIS and others. Glencoe, Illinois: The Free Press, 1953. vii, 288 pp., illus. $3.50.

Both these volumes concern themselves with attempts to produce culture change

within a restricted rural region in Latin America. In methods, approach, and general philosophy, they represent two different although complementary paths.

Fisher's study is a description of a Mexican Government basic education project which he observed for a year as an auxiliary participant in charge of an American Friend's Service Committee group and which he visited again two years later. The account consists of an introductory section, a description of the culture and region of the Santiago Valley of Nayarit, Mexico, and a description of the first year's activities and problems. This is followed by a brief review of culture change theory which is then applied to the Nayarit materials; the study is concluded with notes on the revisit two years later. A transcript of a seminar held at Tulane by five staff members with Fisher is appended; it would have been enhanced had more of the staff members read the report. Fisher's orientation is that of the general anthropologist, but one who prefers his field work to be in the realm of action rather than in pure research. Most of his analysis is of specific problems met in the course of the work and of how anthropological culture change theory helps to interpret them. No use of measurement techniques is made by the author, although he does refer to some before-and-during studies made by Radvanyi. The collecting of data evidently suffered somewhat from the fact that Fisher was an action worker, but this type of work has other assets. In the reviewer's opinion his conclusions are generally sound, and the monograph can be helpful to persons involved in directed culture change projects.

Turrialba is the work of 16 men (six Latins, nine North Americans, and one Finn) connected with the Interamerican Institute of Agricultural Sciences at Turrialba, Costa Rica. Turrialba has been the center for training of field workers in extension work and basic agronomy, and the Turrialba region has served as a field laboratory. Unlike the Nayarit report, this does not deal with any specific attempts to change culture but with a series of studies carried on as preliminary measures to attempting the introduction of such change. The chapters are very uneven both in content and goal; some seem designed to demonstrate rural sociological techniques and to reveal aspects of the local social structure of some of the plantation settlements and rural communities around Turrialba; some deal in broad terms about the entire nation. As a result, the reader is left without as clear a picture about Turrialba as is provided by Fisher on his Santiago Valley region. On the other hand, many of the Turrialba studies provide him with a much sharper picture of the social structure of that region than Fisher does of Nayarit; rural sociological techniques of social structure analysis are put to effective use in *Turrialba*.

The chapters in *Turrialba* are each on a different "system": informal social systems, economic systems, health systems, religious systems, educational systems, agricultural extension systems, and political systems. In addition, there are more specialized studies on levels of living, demography, and the ecological basis of the social systems. There is very little evidence of an anthropological or cultural viewpoint in any of these chapters except that by Thomas Norris. The chapter on Levels of Living uses the term *culture* in its common and nonanthropological sense. In spite of occasional remarks to the effect that the family and kin systems must be important because they seem to raise their heads in the other systems described, the editors evidently miss the cue and devote no space to either.

Both *Turrialba* and Fisher's study contain a set of "hypotheses" or "propositions" about the nature of culture change. The latter are derived from the author's review of culture change theory and are applied to his data. In the former, they come in the last chapter as "examples" of hypotheses which might be tested. The derivation of Fisher's propositions is made quite clear, as is their application to his data; neither the deriva-

tion nor the application of many of the hypotheses suggested in *Turrialba* are so clear, and some suffer from lack of definition.

In general, Fisher's study is strongest in general anthropological approach, in stating his goals and adhering to a path which achieves them. It is weak in that it does not provide measurement of many things which would be better measured. The Turrialba study is strong in its techniques of measurements, but weak in its general composition, being actually a combination of parts of a monograph on the social structure of Turrialba, of short essays on Costa Rican government, educational, and extension systems, and some reflections on culture change; none of the three are well integrated with each other. One suspects that *Turrialba* suffered from too many editors.

While the reviewer found Fisher's study more enjoyable reading and received a better general picture of the Nayarit problems and the culture, the Turrialba study provides the reader with specific examples of the utility of rural sociological techniques. Both these volumes will serve people from disciplines bordering on those of the writers as well as providing comparative data and thinking for their colleagues who find themselves working on similar problems. Neither makes a marked contribution to theory, although the Turrialba study proposes a number of suggestive ideas.

RICHARD N. ADAMS, *World Health Organization (Guatemala City)*

Spanish-Guarani Relations in Early Colonial Paraguay. ELMAN R. SERVICE. ("Anthropological Papers, Museum of Anthropology, University of Michigan," 9.) Ann Arbor, 1954. 106 pp. $1.00.

This book traces the development of relationships between the Spaniards and the Paraguayan Guarani Indians from 1537 to 1620. 1537 marks the beginning of Spanish colonization in Paraguay, and by 1620, the author believes, Spanish-Guarani relations had already reached the phase of mutual adjustment and blending which characterize contemporary Paraguay.

Service's most lucid account is based upon four sets of ordinances from the sixteenth and early seventeenth centuries. There is of course the problem of interpreting ordinance texts and weighing testimonies of contemporary witnesses in such a way as to detect biases which might distort and invalidate conclusions. Fortunately the author reveals an assuring alertness to hidden bias, and he is certainly not guilty of overrating facts to accommodate preconceived notions.

Paraguay must have been a bitter disappointment to the few hundred Spaniards who settled there to find some of the mineral or agricultural wealth which had inaugurated an era of great prosperity in the Andes and in Central America. Poverty and lack of security threw Spaniards and Indians together. A trading or middle class could not develop because there was nothing to trade with, and the economy remained on the subsistence level. As the country was economically unattractive, the handful of Spaniards was left alone. Lack of white women, weakness of church control, and the easy-going ways of the Guarani Indians favored white-Indian polygyny and led to the emergence of a large mestizo population with a mixed cultural heritage. The mestizos soon outnumbered the Spaniards who, at the end of the sixteenth century, were rapidly dying out. While elsewhere the *encomienda* tended to keep the cultures apart in Paraguay, it "fostered, rather than retarded, the assimilation of the Indians into the dominant Spanish culture pattern" (p. 92). Absorption of the Indians was further facilitated by structural characteristics of Guarani society.

The reviewer was impressed by many close analogies to regional situations in cer-

tain parts of Brazil. Many of Service's findings apply *ipsis letris* to S. Paulo, including the retention of the Guarani language, which was spoken almost until the middle of the eighteenth century. At any rate, the common notion of Paraguay as an "Indian country" is as false as it would be to classify Brazil, or any part of it, as something comparable to Bolivia, Peru, or Guatemala.

A few minor points seem objectionable. Thus one can hardly accept the statement that "the Parana Basin to the east [was] occupied almost exclusively by Guarani" (p. 13). The terms *Paulista* and *Mameluco* are not interchangeable. A *Mameluco* is a white-Indian cross, yet not all *Paulistas* were *Mamelucos*. Furthermore, there seems to be little credibility in Lopez de Velasco's statement that there were at one time four hundred thousand Indians in the region of Asuncion, or anywhere in the Chaco, according to Service. Neither the level of the native economy nor the low degree of political integration of the native societies warrants such an assumption.

It would seem that the yield in anthropologically significant data was rich enough to encourage further studies of this kind.

EMILIO WILLEMS, *Vanderbilt University*

Región Meridional de America del Sur: Periodo Indigena. FERNANDO MARQUEZ MIRANDA. ("Comisión de Historia," 71.) Mexico: Comisión de Historia, Instituto Panamericano de Geografia e Historia, 1954. vi, 240 pp.

Tenth in a series of volumes projected by the Pan American Institute's American History Program, this book is devoted to the aboriginal populations of Argentina, Chile, and Uruguay. The primary intent seems to be that of reviewing archeological and historical sources appropriate to a more precise culture-area mapping of the region in question. Basic descriptive materials, in declared difference from the *Handbook of South American Indians*, are neither summarized nor arrayed in a fashion which represents a theoretical departure. The book pretends, instead, to be a syllabus for an advanced course which the author would like to teach concerning unsolved problems of spatiotemporal distributions. An abundance of such problems is brought to light, all of which underscore the drastic interdependence of archeology, history, and ethnology, in a region where each of these disciplines holds a separate piece in a common puzzle.

The syllabus-like organization has resulted in a strangely complicated format. Forty-eight outlined *themes* are presented, most, but not all, of which are followed by discursive sections, which are in turn followed by bibliographies. The *theme* outlines are conspicuously redundant, consisting mostly of mere subject headings such as *Area, Geography, Natural Resources, Art, Anthropological Characteristics, Language, Sources*, etc., to a dozen or more lines in some cases. Moreover, the discussions and the bibliographies which follow are in most instances totally irrelevant to these lists of subjects.

In spite of its peculiarities, the work constitutes a valuable guide for future research. Most of the meager archeology of the region is referred to, if not summarized; promising sites are indicated in many cases, and specific as well as broad problems of ethnohistorical connections and origins are posed throughout. Interpretations of objects of doubtful use are discussed. Published chronicles and travelers' accounts known to exist but which have been lost to modern scholars are mentioned in the hope that the search for them may be intensified. Occasional reference is also made to the state of physical anthropology for certain subregions, and some features of the contact period, colonial acculturation, and the modern demographic situation are also touched upon. Although these inclusions are not uniform for the six subregions treated—northern Chile and

northern Argentina, northeast Argentina and Uruguay, the Chaco, the Pama, the Auracanian area, and the extreme south—the irregularity essentially reflects the data's rather than the author's foibles.

MARVIN HARRIS, *Columbia University*

Nouville: Un Village Français. LUCIEN BERNOT and RENÉ BLANCARD. ("Travaux et Memoires de l'Institut d'Ethnologie," LVII.) Paris: Institut d'Ethnologie, 1953. vii, 447 pp., 17 figs., 4 plates. $7.00.

We have known for some time that the UNESCO organization had planned a series of intensive European community analyses, so the present volume, published by the University of Paris with UNESCO concurrence, is testimony to the seriousness of this intent. This work, carefully and thoughtfully performed, ought to be widely appreciated in both Europe and America.

Nouville is the pseudonym of a "frontier" village, actually in Normandy but so cut off from its parental province by the Forest of Eu that it looks toward Picardy, or even toward the Belgian frontier. The culture and people are, therefore, marginal. Nouville is also an isolated village, despite its proximity to great interprovincial highways and to one of those powerful forces for acculturation, a middle-class beach resort, patronized by metropolitan excursionists. Such enclaves do exist, and very commonly near the grand lines of communication. That they may constitute a worldwide phenomenon is a suggestion worthy of thought and explanation by social scientists, for isolation is not entirely a matter of space or geographic inaccessibility.

The monograph contains detailed considerations of the geographic setting, history, demography, economics, and such peripheral fields. Although they are not as well developed as the specialists might demand, these half loaves are far preferable to no bread at all served up in too many studies in social anthropology. The peripheral sections were not included simply in the interests of thorough workmanship. The writers, quite correctly, feel that they have established a solidarity between the sociologic, historic, psychologic, and geographic elements in the lives of the villagers. They feel that such phenomena have had a formative impact on the modal personality of the Nouvillois, although they do not explain the deep contradictions in the village life, nor the depressing negativist spirit of the place.

The writers consider that theirs is primarily a work in ethnography and psychology "in the French sense of the terms," and it is in those fields where they appear at their best. They are particularly skillful in the field of pure sociology, however, and their section on attitudes and values is especially admirable. The summary of the poll of peasant opinion on America and Americans, for example, is illuminating on the "what they think" level, and somewhat alarming from the "why they think that way" aspect.

Nouville is not an exclusively agricultural village, for a glass factory was founded there in the seventeenth century because of the Eu Forest charcoal. Coal supplanted that fuel, and later the machine moved in, but the industry has remained around a population with "know-how." Semi-industrial villages are new to America but old in peasant Europe. In any event, the researchers were provided with two contrasting segments of the population which differ from one another in ethos far less than one would expect. Cultivator and worker surely agree that their village is one in which nothing succeeds and, like their manorial ancestors, that *la liberté consiste à faire ce qui n'est pas defendu.* One would have expected the glassworkers to have more contact with the outside world and hence to constitute a nucleus for acculturation, but such has not taken place. Poverty is perhaps the explanation.

A marked weakness and paucity of social groups is noted for both peasants and glass-workers. The factory people, crushed by a miserable life, have real need for unions but make no effort to form them. The cultivators are far too withdrawn to form co-operatives. It is the too numerous petty official class which forms these, which, however, are centered in Paris and not in Nouville. The prosperous farmers and landowners unite enthusiastically on a class basis but without what Americans call "community spirit." The church has meant little to the peasant for centuries, since religious lassitude was noted for the region by St. Vincent de Paul. It is again the prosperous who are pious.

The population was sampled and exposed to a battery of Rorschach, Murray, and Craven tests by experienced psychologists but the results were far from conclusive, as more than one anthropologist has found for his particular field of study.

Nouville babies are amply nourished and with great regularity. Even fathers rise in the middle of the night to help, which the authors equate with the peasant attitudes toward domestic animals. Toilet-training, however, is lax and belated, which, though not stated, may likewise have a barnyard origin. The universal fear and dislike of pregnancy on the part of the mothers, however, might produce a guilt of unwanted maternity to be resolved by meticulous feeding, toilette laxity, and overprotection. Disciplinary attitudes are hard to pin down, and they explain only the negativist ethos of Nouville. The idea seems to be to protect the child (swaddling was universal until quite recently) by restraining him and teaching him to avoid life. The peasants seem to suffer from insecurity and confusion, and produce passive, tranquil, unaggressive children with few interests and little disequilibrium.

An interesting contrast between peasant and worker family structure was observed. The peasant family, being the sole center of economic activity and protection, having both a father and mother at home and productive, is biparental in orientation and of great emotional importance. The glassworker father, however, spending his daylight hours as an anonymous entity, making a product with a machine of equal anonymity, leaves the mother as the center of economic protection and child-training. Most interestingly, the factory boss has stepped in to act as paternal surrogate, correcting or commending the child, upholding the mother's disciplinary hands and commending or reprimanding her, and otherwise acting as a third parent. Thus the child is in a way a pupil of the factory while young, an employee thereof at adolescence, and is throughout life under an employer who knows his every fault and virtue and who is not loath to comment on either.

In Nouville we have what might be a study of the peasant class as a whole, or of France as a whole. It is far from a cheerful or promising picture painted here. There are those who object to the flower-in-the-crannied-wall type anthropological study. There are those who doubt the value of writing encyclopedically of a tiny commune. The reviewer is not one, however, and wishes that we had a library of monographs like the present one covering the whole of Europe.

HARRY HOLBERT TURNEY-HIGH, *University of South Carolina*

Die Religionsquellen der baltischen Völker und die Ergebnisse der bisherigen Forschungen. HARALDS BIEZAIS. Uppsala: Almqvist & Wiksells Boktryckeri, 1954. 65 pp.

This annotated bibliography surveys the literature on the Baltic religions, paying special attention to untranslated modern works in Lithuanian and Lettish. Common reliance on medieval writers is weakened by their ignorance of the Baltic languages. Work by Baltic scholars dates mainly since 1920 and is mainly descriptive in character. While the Letts retained their pre-Christian tradition well into the eighteenth century,

their superlatively rich folklore (400,000 recorded folksongs, 800,000 variants) remains unstudied for traces of survivals. Anthropologists may be interested in E. Šturms' attempt to combine archeological evidence with evidence from folklore and historical sources.

ERIC R. WOLF, *University of Illinois*

The Monguors of the Kansu-Tibetan Frontier: Their Origin, History, and Social Organization. LOUIS M. J. SCHRAM, C.I.C.M. Introduction by OWEN LATTIMORE. ("Transactions of the American Philosophical Society," n.s. 44, Pt. 1.) Philadelphia, 1954. 138 pp. $2.00.

Father Schram had a close relationship with the peoples of northwest China over a period of four decades, and from 1911 to 1922 he lived in the locality where the Monguors dwell. Coupling his observations made in the field with unceasing library studies, he comes excellently prepared for the presentation of Monguor historical ethnology and present-day social organization. The present study is the first of a series of three volumes to appear in the Transactions of the A.P.S., later volumes to treat of the religion, history, economy, etc., of the Monguors.

The Monguors are a people of Mongol origin who have been subject to a high degree of acculturation. They live in a region occupied by Tibetans, Chinese, and Chinese Moslems. Schram briefly surveys the geography and ethnic milieu of the Monguors in Chapter I; he outlines their history in Chapter II, in which he has scoured the Chinese and Western literature for references to the Monguors and to the Hsining region where they live. Chapter III is devoted to the organization of the Monguor clan, the largest social unit treated; Chapter IV deals with the next smaller unit, the village; Chapter V is devoted to the next smaller still, the family. There is a brief summary of the economy in Chapter VI, a topic to be covered more extensively in a future volume; here the emphasis is on the historical development of the economy. There is a final chapter of conclusions, a historical appendix on the problem of ethnonymy regarding the Sha-t'o Turks, a note on the political and economic significance of Lama Buddhism, and an index.

There is not a great literature on the Monguors. Schram wrote a lengthy treatise on their marriage practices in 1932 (*Le Mariage chez les T'u-Jen de Kan-sou*), and his colleagues of the Scheut Mission, Fathers Antoine Mostaert and A. De Smedt, have given us a systematic treatment of the phonology, grammar, and lexicon in a number of excellent publications. Mostaert has described the Monguor dialect as an archaic Mongol tongue (p. 28). It has borrowed heavily from Chinese and to a lesser degree from Tibetan and Turkic. This may be seen from the list of kinship terms given on p. 85. Of the 30 terms listed, 20 are Mongol, 6 are Chinese, 2 are Turkic, and 2 are Tibetan in origin. The kinship terminology forms a reliable index of the extent of acculturation of the Monguors. The dialects of the Monguors and of other Mongol speakers are not mutually comprehensible.

Schram has added importantly to our knowledge of how social groupings are formed in the seeming disorder of shifting loyalties and structures on the Asiatic steppes. Societies are continually coming into being and going out of being. There is a non-organization or lack of total organization out of which either permanent local regimes or impermanent universal regimes, such as the empire of Chingis Khan, emerge. The Monguors reflect both tendencies, for, as Schram puts it, "The grouping of Mongol and Sha-t'o [Turkic] tribes which resulted in the formation of the Monguor people was effected in the course of the Mongol conquests in the thirteenth century" (p. 29).

The processes which Schram describes have a general application, because similar phenomena are to be observed in the formation of the Buryats, the Kalmuks, the Kazakhs, the Kirgiz, and many other peoples of steppeland Asia, even though the development of these peoples need not have reference to the same historical event.

The Monguors are organized into a system of seventeen clans, each with a separate territory, a distinctive patronymic, and a separate set of religious rites. Each clan is ruled by its own clan chief, a figure who bears the Chinese title of *t'u-ssu*. The *t'u-ssu* system is a device of the Chinese polity for the administration of the non-Chinese peoples of the northwestern, western, and southwestern border areas. While the Monguor clan names were originally Mongol, they are now all Chinese. Schram distinguishes between these *t'u-ssu* and a tribute-paying chief. Since the latter is a more independent relationship, the distinction is of considerable importance. The Chinese have long had a policy of establishing native rulers as local officials. The tribute-paying chief is more often found in history along the northern borders of China where Chinese control of the Mongol and other nomads was far more tenuous.

The Monguors are today almost entirely farmers, having given up pastoralism to a great extent. They have moved from the nomadic tent to the sedentary farmhouse.

As for the lesser units of social organization, the village is a joint territorial and consanguineal unit. Leadership in the village is granted to an elder by vote of a council of elders. The Monguor family is an extended family, patrilineal in organization, as are all Monguor consanguineal institutions. Marriage is patrilocal, and the levirate is practiced. Polygyny exists but is largely an economic matter. A point of some theoretical importance made by Schram in his earlier publication regarding the levirate is that widows may marry either the elder or the younger surviving brother, hence senior as well as junior levirate, with the junior levirate more common. The position of head of the family passes by direct primogenitary succession, i.e., to the eldest son of the first wife. If an extended family breaks up, the eldest son retains the major share of the corporeal property as well as the incorporeal (he is the religious leader of his small kin-group).

Schram's work supplies excellent data on a specialized facet of the general problem of acculturation. In this and many other parts of Asia there are cases of close cultural contact between peoples over long time-periods, amply traced in the historical records. Here is a series of problems related to intensive, long-term acculturation, the examination of which can broaden the general theory. Similar problems exist among peoples of southwestern China, throughout southeastern Asia, among India's peoples, between Iranians and Turks, and so forth. For one thing, how do social entities remain in being as peoples under such conditions? The study of the peoples of Inner Asia has broadened our knowledge of how groups come into being. How they continue in being is an equally important theoretical problem, and one with modern-day practical implications, for related to this latter problem is that of the many-colored nationalisms of Asia.

Schram is to be congratulated on a careful, learned, well-formulated study, fulfilling the promise of his earlier treatment of the Monguors.

LAWRENCE KRADER, *The American University*

Among the Samoyed in Siberia. KAI DONNER. Translated by RINEHART KYLER. Edited by GENEVIEVE A. HIGHLAND. ("Behavior Science Translations.") New Haven: Human Relations Area Files, Inc., 1954. xiv, 176 pp., 62 drawings. $1.75.

This is an excellent translation from the German of *Bei den Samojeden in Sibirien*, published in Stuttgart in 1926. The book is a by-product of two trips which Donner

made to the Samoyed (1911–13 and 1914) to gather linguistic data. While much of the volume is devoted to accounts of personal travail in passages reminiscent of Freuchen, there is a great deal here of interest to the anthropologist. Obviously a perceptive man, Donner writes (p. 68):

> The Ket Samoyed lives frugally and has few worldly goods. Yet his world is rich, large and beautiful for his existence is not solely bound to the temporal. With the help of song and legend he is still able to create a realm of fantasy in which he lives just as full a life as anyone else on earth.

Although a linguist working forty years ago, the author offers a number of "methodological" suggestions which the present-day ethnographer will find congenial (p. 74):

> One cannot get a correct picture of the religion of a people by the testimony of one individual, just as one is not able to do this with the language of an entire people. And it is specifically in religious matters that what is individual plays such an inordinately large role, most often underestimated.

The value of the book lies in part in Donner's approach to his "dying tribes," for it is this quality which raises his report far above the usual "incidental ethnography." He says (pp. 116–17):

> . . . one [could not] learn to understand the subtleties of their real life, as little as one could ever learn to devote one's full interest and love to the natives if one does not study them as one's equals. If one wants to study the good and the bad in a human being one must try to live their life in their way. And what could be more interesting and productive than to plunge oneself into this infinitely variable and infinitely rich study?

Much of the book is tinged with nostalgia and a kind of bitterness, for Donner felt keenly the passing of the native way of life. One may add the Samoyed to the roster of peoples to whom contact brought declining populations, loss of language, disease, and despair. The accounts of the highly acculturated Samoyed towns will be unfamiliar only in content, for the reader will recognize in these passages kinds of changes already well known to him.

ROBERT J. SMITH, *Cornell University*

Handbook of West African Art. WILLIAM R. BASCOM and PAUL GEBAUER. ("Popular Science Handbook Series," 5.) Milwaukee: Milwaukee Public Museum, 1953. 83 pp., illus. $.75.

Les Civilisations Africaines. DENISE PAULME. Paris: Presses Universitaires de France, 1953. 126 pp., 5 maps.

Contribution a l'Étude de l'Histoire de l'Ancien Royaume de Porto-Novo. A. AKINDÉLÉ and C. AGUESSY. ("Memoires de l'Institut Français d'Afrique Noire," 25.) Dakar: IFAN, 1953. 168 pp., 8 plates, 2 maps. 1000 frs.

The Milwaukee Public Museum is to be congratulated for presenting us with a real bargain. Their *Handbook* is a handsomely produced collection of pictures of a great art at a price that will not strain the slimmest pocketbook. And as if this were not enough, the two essays, by Bascom on the Guinea Coast generally, and by Gebauer on the Cameroons, actually make sensible reading—a most rare quality in art books. Bascom's article, in particular, makes a very useful popular introduction to Guinea Coast society and culture. He explains the principal regional differences in art style and the social function of many of the illustrated masks and other pieces. One can only applaud his calm assumption that West African art developed in West Africa. He does seem unnecessarily conservative in doubting that the facial striations on the famous

Ife bronze heads (beautifully pictured here) are "strictly realistic" (p. 14). At one time or another I have seen at least ten old men—in Ibadan, to the west of Ife, and in Ibo country to the east—who had full facial linear scarification, differing only in detail of pattern from that on the Ife heads. In parts of Ibo country such scarification is associated with the taking of important titles. I believe the Ife heads to be entirely realistic in the matter of facial striations.

I have observed the Janus-faced dance-masks, here illustrated from the "Cross River Area" of the Cameroons, as far west as Ito of Egede, in Benue Province. They were owned by age-grade societies and danced as a symbol of the society (p. 51).

There is a need for a good introductory book to Africa, but *Les Civilisations Africaines* does not fill it. The fundamental assumption in its approach is that Negro Africa is a cultural void, waiting passively to be filled from the outside. This is seen in the plan of the book, the First Part of which, Historic Data, begins with a chapter on "Africa and the Mediterranean World," attempting to derive Negro African culture from the north, via the Egyptians and the Carthaginians. The second chapter, "Islam in Africa," is a fair summary of the history of the spread of Islam in this region and of the various conquests and counterconquests in the Sudan. The third chapter, "The Black African Kingdoms," surveys the whole problem in six pages and then says that the rise of the Zulu kingdom under Chaka provides us with the model for the formation of all the African states, " . . . almost all the history of which we do not know" (p. 53).

The author's faith in the old "Hamitic" explanation of everything interesting in African culture is explicit. She says (p. 14), "In fact, the history of Africa south of the Sahara is in great part the history of its penetration in the course of the ages by Hamitic civilization. . . ." This statement might well be taken direct from Seligman's *Races of Africa;* and in general Paulme's book does not rise above the level of this older, badly outdated work. She does not go so far as Seligman, however, in seeing "Hamitic" mixture in every population that does not correspond physically to the extreme- "typical" Negroid.

It is hard to see how, if Paulme were acquainted with any of the literature on Ashanti and the Gold Coast, she could have made the statement: "In no indigenous African society did gold serve as a money of exchange. The Blacks of the Guinea Coast, who have certainly seen pieces of gold in the hands of merchants . . . never imagined that this metal could circulate for everyday uses between commoners" (p. 120).

The last part of the book is a 60-page summary statement of the principal ethnic groups in Africa. The bibliography does not contain a single original monograph on an African people. The book is a summary of summaries.

It is a relief to turn to the work of two Africans, a history of the kingdom of Porto Novo, along the coast of the present French colony of Dahomey. Their beautifully printed monograph is based upon and amplifies their translation of an older work in Yoruba which Akindélé Akinsowon, the father of one of the authors, published in 1914. The present work is in excellent French.

The first part of this monograph gives at some length the traditional history of the founding of the royal lineage and of the founding of the kingdom of Tôfa (Porto Novo) by Tè Agbanlin, a son of a king of Allada, which they say took place in 1688. These stories are full of trickery and bloodshed and read like the rather better documented history of the early Middle Ages in Europe.

The second part, "The Administrative Organization of the Kingdom," tells of the death, election and consecration of the king, whose office is held in rotation by the five royal sublineages which count descent from Tè Agbanlin. The death of a king is con-

cealed for three months. The choice of a new king is subject to the assent of the *Fa* oracle. The consecration of the King is done through ceremonies whose symbolism fixes him with reference to the lineage and to the law. He is ceremonially "reborn" from the senior woman of his lineage. There is a consecration designed to put the king "in contact with all the spiritual forces of the kingdom." There is a ceremony of transmission of power, in which the priest of the cult of the most ancient ancestor gives him various symbols of the executive power, while saying some variation of the following formula: "I give you this cord in the name of the founders of the Aleda people. You will bind every criminal on this earth and no evil spirit may hold you accountable for it." With this the king is given official immunity from vengeance for his executive acts.

A short chapter on the social life of the princes shows them to be subject to little control. Their behavior is best described as delinquent.

There is an excellent discussion of the history and role of the various royal ministers.

There was a sort of Praetorian guard, known as the *laris*, of which there were male and female branches. The king, the ministers, and the principal *vodun* cults had *laris*, who often terrorized the community at large, in the immemorial manner of Praetorian guards.

The fourth part of the monograph gives what is known of the history of the particular kings.

The last half of the monograph gives considerable general information about the family, the religion, and a summary history of several quarters of the city. It begins with a sort of paradigm of the nature of lineage, which adds little to our knowledge of this subject in Africa generally but which is most interesting coming from a wholly African source. Speaking of the goal of polygynous marriage, the authors say, "This polygamy had a precise purpose: to have many children. The child is a work-hand, popularity, wealth" (p. 95).

The work closes with a very welcome list of African words employed, several pages of photographs, and bibliography of Dahomey and the Guinea Coast. Unfortunately this includes only works published in French.

This work cannot be considered a really professional piece of ethnography. It is nonetheless a worthy addition to Africanist literature. It is good to see Africans publishing important works on their own land. Their monograph puts a little-known kingdom securely "on the map."

ROBERT G. ARMSTRONG, *Cincinnati*

Les Gens du Riz—Kissi de Haute Guinée Française. DENISE PAULME. ("Recherches en Sciences Humaines," IV.) Paris: Librairie Plon, 1954. 232 pp., 11 figs., 12 plates. 795 fr.

Denise Paulme presents us with an interesting example of Anglo-Saxon influence on continental European research. Her comments on Kissi culture indeed, though embodied in the frame of a classical monograph, reveal constant interest in theoretical issues raised by American and British anthropologists. Her tribe consists of about 210,000 people, divided into French, British, and Liberian subjects. She visited them as an archeologist, in search of the makers of some celebrated "Kissi stones," and made good for failure in this respect by collecting material of great anthropological value.

Kissi villages are described as carrying on in full independence from each other, though seldom including more than 150 inhabitants each. The author, in her introduction, gives some data on village life, even the plan of a village of 71 residents. However, in stressing the importance of the lineage, she forgets to tell us how the village unit of

more than one lineage, which is the usual case, works on such important occasions as the selection of a chief. No information at all is said to be available on the origins of the existing political system, on the ground that all the past was wiped out, even in the memory of the survivors, by Samori, the Mandingo conqueror, who was only turned out from the region by the French. It is hard to believe that a longer stay among the Kissi would not have thrown some light on this and other matters.

The first part of the book is devoted to economic development. The Kissi today are found to be doing well, in spite of the French government's interference with their rice crop in order to feed the less privileged part of their West African federation. However, the calendar of agricultural activities reveals that the author may have missed something, as her two stays among the Kissi never encompassed the full course of a year. Coming to land tenure, she raises the question of the position of women in a patrilocal society, a matter which provides some of the most interesting points in her book. She finds that women always insisted on individual plots for their gardening, and that their attitude in this respect stands in sharp contrast with the collectivistic outlook of their husbands.

Kinship is the subject covered in the second part. Paulme has found that it is impossible to trace Kissi genealogies farther back than four or five generations. Tribe and even clan, under such circumstances, become a matter of convention, with no stronger foundation for either one than a common name (p. 87). Totems and taboos are attributes of the lineage rather than of the clan, as they may vary among members of the latter group, according to their village. The author sees in them an expression of the uneasiness of man in his relations to gods, dead human beings, and dangerous animals. The question of woman's status is also of importance in this field. It seems well proved that the wife forever remains a stranger in her husband's family, and that easy relationships exist only between her and her husband's younger brother, who also happens to be the husband's appointed successor in the event of his death. Economic developments may result in some social change, because young husbands today have to start married life with a longer stay with their father-in-law, as bridewealth grows bigger and more difficult to collect. This is a point which may be worth another visit to the Kissi, a few years hence.

Religion and related matters are covered in the third and last part of the book. Males and females again are divided on the subject. Women are never admitted to full participation in either their own ancestors' or their husbands' cults, but all wives in a village constitute the membership of special ceremonies, usually held on the bank of a river, not only because they are used to go there to fetch water, but also because water is identified with fertility. "Kissi stones" are displayed on most family altars but their origins remain a mystery, although the author feels that some of these little statues are reminiscent of sixteenth-century Portuguese explorers. Anyway, to the Kissi themselves they only become important when found by chance on the ground, under circumstances indicating the presence in them of dead ancestors. No other features of the Kissi religion seem to be of an unusual character but Paulme has some interesting things to say on the subject of magic.

Common sense, she holds, governs African behavior in the magical field. Due to their poor material equipment and other factors connected with what today's fashion calls "underdevelopment," the Kissi is bound to feel helpless in the face of equatorial nature. Western people may object to faith in diviners and medicine men, but the African way of dealing with them is as logical as can be. These men are kept under constant watch and none of them keeps long in business unless he succeeds in most

cases submitted to his care. There is, moreover, a definite feeling that only witches can know about the ways of witches so that all dealing with them appear a little suspicious, but the ultimate conclusion remains that one has to make the best possible use of their services, in a world which seems to rule out perfection. Pauline excels in this kind of analysis, and it is to be hoped that on some future occasion she will develop her ideas on this and on other subjects more fully than was permitted in a book written for the general public.

JEAN COMHAIRE, *Seton Hall University, South Orange, New Jersey*

African Traditional Religion. E. G. PARRINDER. ("World Religions.") London: Hutchinson's University Library, 1954. v, 160 pp. Text ed. $1.80, trade ed. $2.40.

This small volume is a comparative study of religion in Africa, designed to inform the intelligent general reader rather than to serve the professional anthropologist or to contribute to sociocultural theory. The author's purpose is to delineate "main tendencies" and to emphasize the "great comparative homogeneity of African society" in the religious sphere. Having thus delimited the scope of his study, Parrinder proceeds to classify certain religious forms and to illustrate the similarities and slight variations of these forms from ethnographic materials selected from diverse areas of Negro Africa. For details, one must look to the ethnologies from which he has drawn most of his data.

The book has four principal divisions: (1) Introduction, which discusses the significance of African religion and the idea of a "spiritual universe"; (2) The Pantheons, which discusses ideas of a supreme being and of nature gods; (3) The Social Group, which compares such religious elements as the ancestors, divine rulers, communal and personal ritual, and sacred specialists; and (4) Spiritual Forces, which describes beliefs in magic and sorcery, witchcraft, and the soul and its destiny. A very brief epilogue discusses modern tendencies in situations of contact with Mohammedanism and Christianity, and the present and probable effects of European educational systems and industrialization and urbanization.

As a result of this method of presentation, the reader is confronted essentially with a list of classifications pertaining to "psychic powers" and rituals and religious specialists. The homogeneity of African religion is demonstrated by the way in which many African peoples have conceptions, rituals, or kinds of specialists which fit into these categories. Such treatment probably is of benefit to the general reader, although it does not necessarily contribute to his understanding of the role of religion in culture or of the role of religion in a particular African society. The possibility of such understanding is not improved by the almost exclusive emphasis Parrinder puts upon ideas about the "psychic powers," upon rituals, and upon religious specialists. The paucity of references to religiosocial institutions and the rather incidental treatment of the interrelationship of economic, political, and other social aspects of culture with the religious belief and ritual elements detract seriously from the value of the book. He does not ignore these factors, to be sure, but he does slight them, and they, of course, are as crucial as the religious elements themselves for an understanding of religion, particularly by laymen.

Certain aspects of the author's treatment deserve commendation. He is highly sympathetic to Africans and to their religions. Only now and then do his Christian predilections intrude as he attempts to justify African religions to Christian readers. Like any religious man, African or otherwise, Parrinder views the world as "a spiritual arena." While this does not necessarily give him greater insight than the "agnostic sociologist or psychologist," it does not necessarily restrict his perception. The book ends with a quotation from Placide Tempels: "African paganism, the ancient African

wisdom, aspires from the root of its soul towards the very soul of Christian spirituality." But at times one gets the impression that the author rather wistfully wishes that his readers might aspire at least toward the fundamentals of the "ancient" African world view.

Despite an occasional inconsistent lapse, Parrinder is no evolutionist. He makes no case for an original monotheism. He chastises loose terms like "fetish" and "juju" and "primitive religions," if by that is meant a collection of illogical superstitions held by children of darkness. He lists many beliefs and many rituals held by various peoples in various places, and he demonstrates their similarities.

A study more meaningful and useful to any kind of reader, however, would have resulted from the inclusion of a synthesis of all the elements in one or two functioning religious systems and from much stronger emphasis on the dynamic interrelatedness of some otherwise queer religious customs with the economic, political, and social facts of secular life. Religious homogeneity is to be found even more in function than in form. But if one is looking for short, general summaries of ideas about the supernatural, about ritual, and about religious specialists in Africa, this is a useful volume to have.

ROBERT A. LYSTAD, *Tulane University*

Fijian Way of Life. G. K. ROTH. New York: Oxford University Press, 1954. xvi, 176 pp., 17 plates. $2.75.

This excellent work by a sympathetic and understanding colonial officer is in a sense an acculturation study, inasmuch as it presents the Fijian way of life of today rather than of the past. The author is the son of the late H. Ling Roth, so well known for his works on the Tasmanian aborigines and the natives of Sarawak. The appreciative preface to the present work was written by Sir Ronald Garvey, Governor of Fiji, and like Roth an anthropological student: "In this book he [Roth] shows how the Fijians have accepted many of the gifts that Western civilization has to bestow."

The chapter headings give a picture of the scope of the book: I The Physical Background of Village Life (50 pages); II Land Custom, and the Structure of Society (43 pages); III The Principal Customary Ceremonies (40 pages); IV The Fijian Administration (30 pages). Seventeen plates, thirteen of them with two pictures each, give the reader a fine visualization of Fiji and Fijian life as it is today. Supplementing these are the author's detailed and accurate descriptions of activities and artifacts, which make the book of high value as a functional study as well as an ethnography. Twenty-five years of service in Fiji have given the author a perspective on cultural change, since he has seen much of it in actual transition, not only in the realm of material culture but also in the fields of social and political organization and religion. The author does not attempt to present general principles of acculturative processes, but the book is full of examples of how new traits are incorporated with or have modified the old.

The chapter on "The Physical Background of Village Life" deals not only with the physical geography of village sites but with a vast number of cultural items ranging from gardens, houses, and household equipment to exchange of local products between villages. A great array of factual material is organized in terms of time perspective, thus documenting the process of acculturation. The bases of change and the causes of selective choice are skillfully and graphically presented. This chapter constitutes a masterpiece of acculturative study.

Roth's treatment of "Land Custom and the Structure of Society" is clear and concise: "As far as possible the Fijian terms for social units have been translated here by words descriptive of the composition of the units but those such as 'clan' and 'tribe'

which have particular meanings in ethnographical literature have been purposely avoided because they do not seem to me to be applicable judging from the information at present available regarding the nature and characteristics of Fijian social units. The term 'totem' is, however, used here although perhaps a more apposite expression would be 'tabu object'" (p. 55). One part that women play in modern Fijian life, a part thoroughly understandable to Americans, is exemplified by voluntary organizations administered entirely by Fijians. Two such groups exist for the general betterment of women and children, with a branch of one or the other in almost every one of the 1,080 Fijian villages. One of their valuable objectives is the promotion of child-welfare work among Fijian families, both in their own villages and in industrial centers.

The chapter on "The Principal Customary Ceremonies" deals with the numerous and meticulous details of Fijian ceremonial as it is enacted today and in the recent past. Yet Roth succeeds in vivifying this otherwise heavy material by his light and understanding style and by the introduction of personal observations and anecdotes. Not the least interesting of the latter are those dealing with the embarrassments which accrue to masters of ceremonies who make inadvertent slips.

The final chapter of the book presents a firsthand picture of the operation of the indirect system of government based on native social and political practices which the Fijian chiefs and the British administrators have developed successfully. The author in his official capacities has been one of those administrators who have contributed to the smooth functioning of the system.

At some point, most reviewers make at least a few adverse criticisms. In this instance the reviewer is unable to point out any flaws, blinded, perhaps, by the author's thorough and readable style and by the fine piece of bookmaking which the publishers have produced. The book is one of which the reviewer would be proud to be the author.

E. W. GIFFORD, *University of California (Berkeley)*

ARCHEOLOGY

From Savagery to Civilization. GRAHAME CLARK. ("Schuman's College Paperbacks.") New York: Henry Schuman, 1953. ix, 116 pp., 24 figs., 2 tables. $1.00.

Although legal and perhaps in accord with publishing ethics, the information given on the back of the title page of this book is distinctly misleading. The entire statement runs, "Copyright 1953, by Henry Schuman, Inc. Manufactured in the United States of America." Actually this volume is an unrevised reproduction, apparently by photo-offset, of the English edition published in London by the Cobbett Press in 1946. The Preface and the two most recent titles in the 96-item bibliography are dated 1944. The pagination of the two editions is identical, and the only changes visible to this reviewer are the substitution of the title page, the omission of four plates and of all references to them, and the addition of one brief footnote and an index.

Although it must thus be considered approximately ten years old, with some parts undeniably out of date, the book is well written, readable, and "is concerned with tracing the main steps by which man has emerged from the brutes and has attained to civilization." The entire treatment, however, follows the evolutionistic doctrine of cultural development. Clark acknowledges Tylor as his source for the stages of Savagery, Barbarism, and Civilization, which he modifies, just as Childe in *What Happened in History*, first published in England in 1942, went back to Morgan for the same terminology.

The five chapter headings are as follows: I. Human Origins; II. The Lower Savagery; III. The Higher Savagery; IV. Primitive Barbarism; V. The Origins of Civilization.

Lower Savagery refers to the Lower and Middle Paleolithic, Higher Savagery to the Upper Paleolithic and Mesolithic, the Neolithic is termed Barbarism, as is the later transitional trend toward urbanization, and Civilization is defined as the culture of cities.

Although he reviews a great deal of solid archeological evidence, the author has considerable recourse to interpolation based on the concept of stage development, and in many instances he does not mention alternative interpretations and hypotheses. A characteristic example of this is found throughout his discussion of Paleolithic Savagery. Here there is constant explanatory reference to the small size of the band, which is more than once described as never numbering more than 20 individuals, and usually less. What is nowhere met is the recognition that a migratory or semimigratory hunting and gathering kind of life, when carried on in a harsh environment, does limit the size of the group, but that under more favorable conditions, and particularly where there are large herds of migratory game, larger bands are likely to exist.

Another question that arises is, despite the slowness of change, just how far can one assume complete simplicity of culture on the basis of a surviving tool kit. Both of these are relatively minor points, but there are numerous assumptions and statements of this nature, which, although consistent with the author's orientation and perhaps correct, are certainly very far from settled.

Compared with Childe's *What Happened in History*, this book is more restricted in scope, gives somewhat more attention to the Paleolithic, and treats the entire subject from pretty much the same point of view. It is a readable evolutionistic interpretation of cultural development as derived from archeological sources. During the last ten years, however, there have been new fossil finds and archeological discoveries. Older materials have been re-evaluated, and further advances have been made in theoretical thinking. In this light, the value of reproducing such an unrevised work in a series designed for college use seems highly questionable.

ROBERT W. EHRICH, *Brooklyn College*

Clay Figurines of the American Southwest. NOEL MORSS. ("Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University," XLIX, 1.) Cambridge, 1954. x, 114 pp., 2 maps, 13 figs., 18 plates. $3.50.

The occurrence of human effigies of clay in Southwestern sites has been of particular interest for two reasons: one is the question of the connection between these figures and the more abundant figurines of Mexico; the other is the problem of the significance which these images had for their makers. These points, which have important implications to the culture historian, are thoroughly and carefully considered in this recent monograph completed by Morss.

The first few pages of this report are devoted to a cache of eleven ornate clay figurines found in Utah. The figurines are dated in the eleventh century and related to somewhat similar objects found in other parts of Utah. Description and illustrations are excellent.

There follows a comprehensive summary of all the known figurines found in the U. S. west of the Mississippi River. Thirteen figures illustrate dozens of figurines and give samples of all the major finds. Most of the figures have been illustrated elsewhere, but it is a great convenience to have them all gathered together in this compilation.

Concerning a possible Mexican source for Southwestern figurines, it is concluded that there is some connection but that the picture is much more complex than a simple transfer of ideas at one time and place. Although we would expect the figurine idea to

enter the Southwest through the Hohokam area in southern Arizona, we are cautioned that even this is not certain in view of the early dating of some figures found outside the Hohokam region.

The discussion given by Morss covers the many possible meanings which these little clay images may have. A number of ethnographic references are cited to show a symbolic significance for figurines among various Southwestern tribes. That some of the figurines are associated with witchcraft is clear, but in general it is concluded that a fertility symbolism is indicated. Morss comments (p. 55): "That the female figurines are concerned with fecundity we can easily assume, but they represent the mother rather than the child." My own feeling is that this conclusion has to be used with caution, for two reasons:

1. There is some ethnographic evidence for the use of clay figurines as children's dolls or playthings. Without some evidence in the archeological context, how does one tell the dolls from the cult objects?

2. A fertility symbolism for the female figurines does not explain the frequent occurrence of male figurines in association. The latter cannot be ignored if they occur in the same context, yet it seems a little far-fetched to assume that they are also symbolic of fertility. Where male and female figures occur together a more likely interpretation is the alternative suggested by Morss, that the figures represent particular sets of characters.

I should not give the impression that the question of fertility symbolism is the dominating theme of the monograph; my comments on this point arise out of previous study of this particular question. Morss states in his introduction: "No doubt the precise parts played by these objects in the various cultures producing them varied at least as widely as the interpretations which have been suggested." As distressing as it may be to the archeologist, it seems clear that we cannot accurately reconstruct the cultural meaning of figurines any more than we can recover an unwritten language from an archeological site. The best we can hope for is a well-reasoned approximation to the truth, and the interpretation presented by Morss is probably as close to the truth as we can come with our present knowledge and techniques.

CLEMENT W. MEIGHAN, *University of California (Los Angeles)*

Prehistoric Stone Implements of Northern Arizona. RICHARD B. WOODBURY. ("Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University," XXXIV, 6.) Cambridge, Massachusetts, 1954. xiii, 240 pp., 9 figs., 27 plates. $7.50.

From 1935 through 1939 the Peabody Museum of Harvard University excavated, under the direction of J. O. Brew, the prehistoric and early historic Hopi pueblo of Awatovi together with several small neighboring sites in the Jeddito Valley. Every few years a new volume has been added to the series of reports that have flowed from members of the expedition. This volume, devoted to a record of the stone industry, is No. 6 of the series and is a valuable contribution to our knowledge of the archeology of the Southwest.

Although the specific material that is described comes from twenty-six sites in a limited area in the Jeddito Valley, the volume is a monograph on the stonework of a much wider area, including many of the regions once occupied by the Anasazi. The book is divided into three parts: introduction, description of eight thousand artifacts with a discussion of their origin and distribution in time and space, and a conclusion. Following the conclusion are two useful appendices: (A) the chronological placement

of rooms and levels at Awatovi, and (B) provenience of artifacts found on floors, in niches, etc.

As it would form too bulky a volume were the author to describe each individual artifact, he has divided the material into over one hundred and twenty types. These he has tabulated and studied through time, using as a base the Pecos Classification of stages as indicated by pottery types, and comparing them to material from other excavated sites in the Southwest.

The author was somewhat hampered because the reports on the study of architecture and pottery of the Jeddito Valley sites have not yet been published.

When a great many types are considered, some sort of taxonomic scheme is necessary to bring like objects together for study. The author's scheme makes no pretense of being universally applicable. Having no widely accepted system to follow, the writer has conformed to the general custom of the Southwest: namely, whenever he can identify the function, he makes that the basis of classification by analogy with Pueblo Indian technology. When function cannot be inferred, the author uses nonfunctional terms.

His tables show in a graphic manner the persistence of the traits over a long period with little change. Over the twelve to fourteen centuries represented by the material, in contrast to the rapid changes in architecture and pottery types over the same period, two tendencies are observable: first, specialization and variety with innovations frequently providing alternative processes and techniques without supplanting the old ones and, second, an impressive degree of continuity. For these reasons, stone implements contribute much less than do some other traits in building chronologies but are useful in indicating the daily life of the people and in outlining culture areas.

In stoneworking techniques the Hopi learned little from the Spanish contact. Only three examples are recorded: a large stone ring which supported the baptismal font, three or four gun flints, and some mica flakes cut in geometric shapes. These, however, the author feels may have been Spanish-made.

The one hundred and sixty pages devoted to the description and discussion of the types will form a framework of reference for other studies of the stonework of the Anasazi and neighboring cultures.

The book has a bibliography and an index.

HAROLD S. COLTON, *Museum of Northern Arizona*

The Ruins of Zaculeu Guatemala. RICHARD B. WOODBURY and AUBREY S. TRIK and others. Vol. I and II. Boston: United Fruit Company, 1954. xviii, 324 pp., 297 plates.

This large monograph constitutes a major step forward in our knowledge of the archeological history of highland Guatemala. For a long time investigations in this area lagged far behind those of the Maya lowlands, but with this study, with the Carnegie Institution's recently published reports on Kaminaljuyu and Nebaj, and with Wauchope's *Excavations at Zacualpa*, it might almost be said that the Guatemala Highlands are about as well known archeologically as any of the major areas of Mesoamerica. This is all to the good, of course, for it has become clear that the area played an important role in Mesoamerican affairs at various times.

The present study is the complete report on the so-called Zaculeu Project, which, as explained in a note on the title page, was "one of an extensive program of public service projects undertaken for the nations of Middle America by the United Fruit Company." The well-known and relatively well-preserved ruin of Zaculeu near Huehuetenango

in the western highlands was chosen for the project and received five years of intensive field investigation (1946–51). The purpose of the work was twofold: (1) the straight archeological investigation of the site and (2) the repair and restoration of the ruins for the people of Guatemala as a visible record of their pre-Columbian past. This latter purpose was of special significance for Highland Guatemala, since there were previously no restored or easily observable sites for the edification of the local citizens or for the many tourists who visit the region. It was not, therefore, a project of purely archeological research, but the results in this category are of the greatest importance, and American archeology is vastly indebted to the United Fruit Company for the prosecution of the project and the excellent publication that has ensued.

The two bound volumes of the report are handsomely printed and lavishly illustrated. Line drawings, maps and sections accompany the text in the first volume, while the 128 pages of half-tone illustrations comprise the second. The arrangement of material has been most thoughtfully planned for the convenience of the reader in a format which follows closely the pattern of the Carnegie Institution reports on Middle American archeology.

John Dimick, who acted as director of the project, supplies an introduction to the book which outlines the reasons for selecting Zaculeu as the site to excavate and the progress of the field activities. It is of particular value in recording many details of the organization of the labor force, the methods used in obtaining supplies for the reconstruction job, and other practical problems that will be of value to those planning similar projects in the future.

Chapter II, by Natalie Woodbury, consists of a summary of the written historical information applicable to the Mam-speaking region in which Zaculeu is located. From sources reflecting pre-Spanish history we learn something of the relationships of the Mam nation to that of the Quiche, but the specific site of Zaculeu as a Mam stronghold is identified only for the period of the Spanish conquest. It was the site of one of the last major battles of the conquest when in 1525 Gonzalo de Alvarado laid siege to Zaculeu and finally subdued it. Curiously no evidence of this battle was found in the extensive excavations at the site, and the Spanish accounts provide little descriptive detail of what these excavations show Zaculeu to have been like in just pre-Spanish times.

The next and one of the two major sections of the report is Aubrey Trik's account of the excavations and his complete and admirable descriptions of all the structures, graves, and caches encountered. Closely packed with a tremendous amount of detailed information, the account is well organized and coupled with sections and plans in a manner that presents the complicated excavational picture as clearly, it seems, as one could hope for. One feels quite confident that the excavations were under control at all times and that the recording is accurate and complete. With the need for preserving and restoring all of the architecture, the many interior structures were seen only in part, usually within a single trench cut along the main axis of the building. This trenching system gave the results that were expected from previous experience in Highland Guatemala sites in exposing numerous burials and caches. It was these artifact-bearing features stratified along with the superimposed buildings that gave the basic sequence of four phases into which the history of the site is divided. These phases, identified by somewhat unfortunately difficult names, are fitted into the general Mesoamerican picture as follows: the Atzan Phase is equated with Early Classic; the Chinaq Phase, with Late Classic; the Qankyak Phase, with Early Post-Classic (plumbate, etc.); the Xinabahul Phase, with Late Post-Classic, just before the Spanish conquest.

The second large section, labeled *Artifacts*, is an extremely well organized and com-

plete presentation of the pottery and all other objects found. The many varieties of pottery are handled in the order of their phase assignment and are classified in a manner to allow of their most complete description rather than in a strict typological scheme. This seems logical (considering the preponderance of whole vessels over sherds), has the advantage of allowing a more precise identification of element combinations in particular types, and paves the way for future distributional studies. Many local varieties of pottery occur in the large sample from Zaculeu, but there is a rather unusually large number of types or techniques of wide distribution in Mesoamerica, giving a quite certain correlation of the phase sequence with that of other Highland Guatemala sites, of the Peten, and of Mexico. Artifacts other than pottery are generally what might have been suspected would occur from our previous knowledge of highland sites, but Zaculeu adds importantly to the whole picture of the distribution of many kinds of tools and ornaments in the region and through a long period of time. William C. Root provides an analysis of the metal objects belonging to the last two phases.

To complete my summary of the contents of the Zaculeu report I must mention three final chapters which could have been listed as appendices. Charles Weer Goff provides "Anthropometry of a Mam-speaking Group of Indians from Highland Guatemala." Previously printed in the *American Journal of Physical Anthropology* (Vol. 6 [1948]) it includes measurements and medical data on sixty-one adult males from the vicinity of the archeological site and comparisons with other collections of similar data from the Maya area. A chapter on "Skeletal Remains from Zaculeu, Guatemala" by T. D. Stewart provides a listing of the physical anthropological data and of head deformation, tooth mutilation, and pathology. In the final chapter Charles Goff contributes "New Evidence of Pre-Columbian Bone Syphilis in Guatemala." The history of discussions concerning the New World origin of syphilis is reviewed and two skulls from Zaculeu are considered to show good evidence of syphilis. An unsuccessful attempt to develop a serologic test for syphilis in old bone samples is described.

As competently done and full as it is, this Zaculeu monograph is deficient in its lack of proper comparative analysis and the drawing together of its diverse threads into some judgment of the significance of the work and of the place of Zaculeu in the whole Mesoamerican scene. One can hardly believe that the only summary or concluding chapter to the whole archeological section is a "Summary of Important Traits" in which the phase attributions of the various architectural and artifactual elements are listed. Perhaps one should not be too critical on this score, however, for summaries of Highland Guatemala archeology by Kidder and Wauchope have recently appeared, and some of the members of the Zaculeu staff cannot be considered specialists in the Mesoamerican field. It is true, too, that the artifact section includes numerous comments, but seldom as extensive as desirable, on the known distribution of various traits and elements. There is nothing of this kind, however, for the all-important architectural section, and the reader will have to investigate for himself such things as the apparent resemblance and probable relationships of the Zaculeu stairways to those of Tazumal in El Salvador, for instance, or of the columned buildings to their seeming counterparts in Yucatan.

GORDON F. EKHOLM, *American Museum of Natural History*

Contributions to American Anthropology and History. Vol. XI, Nos. 52–56. Washington D.C.: Carnegie Institution of Washington, 1952. 233 pp., plates. $6.75 paper, $7.50 cloth.

The volume contains five monographs, each with bibliography, and provides a sub-

stantial increment to the large body of basic information on ancient Mesoamerican cultures which we owe to the Carnegie Institution of Washington. As usual in this series, illustrations are plentiful and good, and well explained.

No. 52: Geologic Observations on the Ancient Human Footprints near Managua, Nicaragua is by Howel Williams (31 pp., 11 figs.). It is a fascinating study by a volcanologist, seeking to date the famous footprints. His own field work is supplemented by full attention to nongeologic evidence. The conclusion is that men and bison walked across a mudflow as it was hardening. This was "at least 2000 years ago, but probably not more than 5000 years ago. Certainly their age is not to be measured in terms of tens of thousands of years."

No. 53: Mound E-III-3, Kaminaljuyu, Guatemala is by Edwin M. Shook and Alfred V. Kidder (96 pp., 81 figs.). Here are full descriptions and illustrations of two splendidly furnished tombs and of what was learned of the architecture of the pyramid in which they were placed. A remarkable salvage operation followed chance discovery by brickyard workers in 1947. The resulting data are outstandingly important because an exceedingly rich assemblage of grave goods had been deposited at the top of the largest pyramid at the site, and in late pre-Classic times. Upwards of 455 pottery vessels were accompanied by marble and greenstone vessels, small stone sculptures, mosaic elements, and much else, including sacrificed human victims. Ancient grave robbing accounts for a comparatively modest haul of jade.

These tombs were in the latest of seven superimposed terraced pyramids with stairways, and belong in the latest two phases of the four which are distinguished in the Miraflores period. That, in turn, is the latest of three known pre-Classic ceramic periods. It is thought that the story might be carried backward in time with earlier tombs yet to be discovered. As of the time of the two known ones it is concluded that "in social organization, in the general nature of religous edifices . . . as well as in most aspects of technology, a stage of advancement closely comparable to that of the Classic period had already been reached." The findings tend to substantiate an expressed suspicion that even the great Pyramid of the Sun at Teotihuacan may have been pre-Classic in age.

Conquest Sites and the Subsequent Destruction of Maya Architecture in the Interior of Northern Yucatan is by Ralph L. Roys, with 52 pages and four illustrations, including map. The current program of the Institution involves trying to fill in gaps in the archeological picture of the post-Classic Maya period in northern Yucatan. A desideratum is to make contact, so to speak, with available history and ethnological data for the last century before the conquest. Roys's excellent study of historical sources should be interesting and instructive to anyone interested in the region or in the impact of the conquest on the Indians. It has a further practical purpose. To discover missing Maya sites of the latest precontact period, which was one of political chaos, one should look near modern churches and town plazas—precisely where local officials and citizens are likely to object to digging. It is probable "not only that a large number of towns in northern Yucatan are located at their pre-Spanish sites, but that the present churches and plazas are situated at the ceremonial centers of the old towns."

The Ball Courts at Copan, With Notes on Courts at La Union, Quirigua, San Pedro Pinula and Asuncion Mita, by Gustav Stromsvik, 30 pp., 23 figs. A generation ago it was first realized that ball courts were typical features of Classic as well as post-Classic Maya ceremonial centers. Stromsvik describes a stratification of three of them at Copan, which he excavated 1937–41. The latest, embellished with sculpture and an inscription, has since been restored by the Institution under his direction. Besides the

courts themselves there is some interesting sculpture, including three depictions of the players, garbed for the strenuous game, facing the ball.

Under *Discussion* Stromsvik registers the opinion that the earliest of the Copan courts predates the use of stone sculpture in the valley. I take this to mean it is probably pre-Classic in age. The evidence cited is suggestive, but not claimed to be conclusive. All three courts appear to have been of the same basic "sloping-bench-top" type, which has a wide distribution in the Maya lowlands as well as Maya and non-Maya highlands, though the earliest differs from the others in details. The latest is dated at 9.11.0.0.0 or later, in the Maya chronology, on the basis of damaged inscriptions on the court structures, and on datings of associated stelae. A dating somewhere in the late Classic Maya period would appear to be certain. The series of three ball court structures on one spot is unique and important, and obviously a fine job was done on them.

Pottery from Chipoc, Alta Verpaz, Guatemala (20 pp., 26 figs.), by Robert E. Smith, is a specialist's report defining a Late Classic ceramic phase at one site. Smith hopes that "the Chipoc phase will form an opening wedge in our scant knowledge of the pottery of Alta Verpaz." In a short introduction he divides the province into western, central, and southern parts. Chipoc is in the central zone, which is drained by the Rio Coban, near the town of Coban. The Chipoc phase corresponds to "Chama 3–4" in the sequence worked out by Butler. There is a suggestion that the famous figure-painted "Chama" vases may have started a little earlier than did the Chipoc types, and that they come from the western part of the region, in Rio Chixey drainage.

LINTON SATTERTHWAITE, *University of Pennsylvania*

Dimini und die Bandkeramik. FRITZ SCHACHERMEYR. ("Prähistorische Forschungen," 4.) Wien: Verlag Ferdinand Berger, 1954. 39 pp., 9 plates.

In this paper Schachermeyr offers two alternative schemes for the relative dating of the Thessalian and Danubian sequences. His first equates the Starčevo complex with that of Sesklo, and Vinča A–C with that of Dimini. His second has Sesklo III overlapping Vinča A, and Dimini I and II paralleling Vinča B and C.

In the early Neolithic he sees the Starčevo complex extending southward to include Sérvia and Otzaki Magula. In the late Neolithic he interprets the characteristic Dimini or Thessalian B complex as intrusive from the north and derived from Bükk, Transylvanian, and Tisza elements.

Schachermeyr's conclusions seem at least partly based on the acceptance of some rather questionable sequences and apparently depend on simple migration to explain almost all trait similarities. Many of the resemblances in the sketches and descriptions seem broad, not closely alike, or insufficiently complex for definitive use in this manner. There can be no doubt that some of them are valid, but in the middle Danube Basin, the presumed origin-point of migration, the traits are pretty well scattered and belong to different culture groups. Some may have been diffused to that area, and others seem to have penetrated southward by independent trait diffusion. They do not necessarily represent a transplanted complex or a migration. In many traits Dimini seems closer to the Eastern Painted sphere, while evidences of middle Danube relationships seem much stronger for Macedonia than they do for Thessaly.

It seems certain that the Dimini and Bandkeramik complexes were at least partly contemporaneous, but there is some reason for believing that the pre-Bandkeramik Starčevo complex does not antedate Thessalian B and must equate at least with its earlier phase (see the reviewer's remarks in *Relative Chronologies in Old World Archeology*, ed. R. W. Ehrich [University of Chicago Press, 1954], pp. 108–29). In his *Pre-*

historic Macedonia (Cambridge University Press, 1939), W. A. Heurtley, moreover, suggests parallels between Lengyel and the Macedonian Early Bronze Age.

Most Continental archeologists, Schachermeyr among them, seem to favor a long chronology for the Neolothic period of Europe. Most of the British and Americans believe that a short one is correct. The former school of thought has as its corollary the concept of major culture radiations and diffusion outward. The latter necessitates a reversal of this process with Central Europe playing a more passive, recipient, and peripheral role.

Schachermeyr's thoughtful paper underlines the need for both extensive and intensive archeological exploration and excavation to fill in blank areas and to re-evaluate interpretations based on inadequate older archeological work. In the meantime, the views held by author and reviewer are widely divergent.

ROBERT W. EHRICH, *Brooklyn College*

PHYSICAL ANTHROPOLOGY

The Racial Affinities of the Baganda and Other Bantu Tribes of British East Africa. LAWRENCE OSCHINSKY. Cambridge, England: W. Heffer and Sons Ltd., 1954. vi, 188 pp. 25/-.

This book offers more than its title promises, in that it concerns itself with the physical anthropology of the tribes of Uganda, Tanganyika, Kenya, Zanzibar, and Ruanda-Urundi. The author has collected not only extensive anthropometric materials but also data on blood grouping and sicklaemia. On the basis of these manifold details he attempts to build a racial classification of all natives of East Africa. He proceeds to include the investigations made in other areas of Negro Africa. In this way he is able present a more or less complete picture of the Negro groups south of the Sahara.

Anthropologists are well aware of the numerous large blanks in our knowledge concerning the African races, and any contribution in this large field of investigation is appreciated. In justifying his attempt at a new racial grouping, our author discusses first the deficiency of several classifications of other authors (Jonston, Montandon, Eickstedt, Seligman, Coon, Vallois) and then compares these with his own. His scheme comprises seven sections: Bambutomorphs, Congomorphs, Bantomorphs, Nilotomorphs, Hamitomorphs, Nilohamitomorphs, and Mulattomorphs. His technique of measurement is that of R. Martin, as expounded in the well-known *Lehrbuch* (1914, 1928). As indicated by the title, this treatise deals mainly with the somatometric characteristics of the Bantomorphic tribes in Uganda and the neighborhood. It is then shown how these tribes differ from other large racial groups in Africa. For the sake of clarity and convenience, tables of comparative data from many African districts are added, enabling the discussion of each somatological character.

As far as anthropological methods are concerned, it is worthy of note that the author refers to certain body indices, namely, those which express various body dimensions and the extremities, as a percentage of stature. Dr. Boné had "calculated the differences between stature and trunk indices in the Pygmy and Forest Negro series of Gusinde, . . . also several of these differences for the writer's [Oschinsky's] Batutsi and Zanzibar Arab series and has obtained similar results. . . . Schultz comes to the following conclusions [and also his disciple, Dr. Boné]. 'To refer the main dimensions of the limbs and head to stature, as customarily done, is misleading since stature itself is so largely dependent upon the length of the lower limbs, the latter being the very dimension which differs most in the series under comparison' " (p. 17).

As far as the racial relations between the seven large groups are concerned, the author presents the following conclusions:

> The Baganda differ from the Batutsi in almost all the absolute and relative dimensions of the body. . . . The Uganda Bantomorphs in general represent an intermediate racial type between the smaller Congomorphs and Bambutomorphs, whose body proportions show many interesting parallels with those of Europeans and the taller Nilotomorphs, Nilohamitomorphs, and Hamitomorphs. . . . The East African Hamitomorphs are to be considered as typical Negroes. The Nilohamitomorphs seem to be a mixture between Nilotomorphs and Northeast African Hamitomorphs, with a much greater percentage of Negro blood than the Hamitomorphs as their body and facial proportions indicate [p. 128].

As stated, these conclusions are rather general, notwithstanding the fact that a good number of measurements and morphological properties served as the starting point for that classification. Important details of the latter cannot be discussed due to limitation of space; they would, in my mind, change somewhat the author's general conclusions.

At least a short commentary may be added concerning the so-called Bambutomorphs. Since an accurate definition of none of the seven large groups is presented—a deficiency which renders difficult the assignment of particular tribes to this or that larger group—I assume that the term "Bambutomorphs" embraces all tribes of food-gatherers in the forest zone of Central Africa characterized by their small stature for whom I have introduced the generic term "Twides," subdivided into three main groups (cf. M. Gusinde, *Urwaldmenschen am Ituri* [Vienna, 1948]). The term "Bambuti" is never used by the Ituri-Pygmies among themselves and should, therefore, be dropped. I am under the impression that our author is not well acquainted with the anthropological differences of the above-mentioned three main Pygmy groups. In his "Literature Cited" are not mentioned the voluminous work published by P. Schumacher, *Die Kimu-Pygmäen* (2 vols., Brussels, 1949/50) and my complete monograph of the same *Twa Pygmies in Ruanda* (Vienna, 1949); and two important articles of general character by H. V. Vallois, "New Research on the Western Negrillos" (*American Journal of Physical Anthropology* 26 [1940]:449–79) and by Eugen Fischer, "Über die Entstehung der Pygmäen" (*Zeitschrift für Morphologie und Anthropologie* 42 [1950]:149–67). Our author is incorrect in stating, "The Babinga are, in fact, a parallel development in the West of that Congomorph-Bambutomorph racial mixture which can be observed on the eastern borders of Pygmy land . . . of the Eastern Belgian Congo" (p. 27). Vallois (*op. cit.*, p. 469) has previously reported, "The Eastern Negrillos, as well as the Western Negrillos, represent two different forms of their race; one is taller than the other and each has its own characteristics." I am able to corroborate this statement by new discoveries. It is likewise incorrect to categorize the Batča as Congomorphs, since they belong to the Western Twides.

In spite of some inaccuracies, every anthropologist is indebted to our author for having gathered a very considerable amount of information concerning the racial properties of numerous African groups.

MARTIN GUSINDE, *Catholic University of America*

FOLKLORE

Miti e Leggendi, III. America Settentrionale. RAFFAELE PETTAZZONI. Torino: Unione Tipografico–Editrice Torinesi, 1953. xx, 576 pp., 22 plates (7 in color), 3 maps. *Lire* 4600.

This is the third volume of what is to become a definitive collection of non-Euro-

pean myth and legend. The first volume, covering Africa and Australia, appeared in 1948; the two remaining will cover Asia, Oceania, and Central and South America. Volume IV will contain a much-needed index to the whole series. The editor, who has just retired from the chair of History of Religions at the University of Rome, has poured into these books his broad and accurate knowledge, and clearly no student of primitive myth or literature can dispense with this handsome, inclusive, and comparatively inexpensive collection. Though text and comment are in Italian, Americanists may easily refer to the original English translations and read the brief but lucid explanatory notes, which appear thoroughly sound in ethnographic content.

This volume, which comprises the mythical tales of American Indians north of Mexico, is certainly the most extensive selection of the kind in print. It supersedes the earlier books of Brinton, Curtin, and Krickeberg, and deserves close comparison with Stith Thompson's *Tales of the North American Indians* (1929). Since the plan of the two works is somewhat different, they will both remain basic texts on the anthropologist's shelves. Thompson cannot of course dispense with myth, but his basic interest is in the folk tale in general, and in the relationship between American and European tales. Pettazzoni, whose prime concern is with myth, must have been clearly aware of his role as complement to Thompson, for of Thompson's 96 and Pettazzoni's 286 tales only four duplicate the actual document, though there are several predictable repetitions of major theme, such as Star Husband, Arrow Chain, and Flood. Thompson classifies by theme: myth, otherworld journey, hero, trickster, animal spouse, *märchen*, and biblical parallel. Pettazzoni classifies instead by ten major linguistic, ethnographic, and geographic groupings: Eskimo, Pacific Northwest, California, Athapaskan, Algonquin, Sioux, Caddo, Iroquois, Gulf or Southeastern, Pueblo and scattered Southwestern tribes. Thompson, perhaps because of his interest in old-world parallels, has an unusual preponderance of tales from the Algonquins of the East Coast first colonized by Europeans, and from the Pacific Northwest with its undoubted folkloristic commitments to Northern Asia. Pettazzoni does not neglect these areas, but he also provides a rich new body of material on the complex California area and a significant selection from Sioux and Caddo. In most cases his source material does not bulk largely from collections published after 1929 and therefore unavailable to Thompson, except for the Eskimo and the Gulf and Pueblo groups, which apparently are the areas which have attracted the maximal effort of the last twenty-five years and which still provide challenges for future collectors.

For one dependent on foreign libraries Pettazzoni shows remarkable knowledge of the Western literature, and many heavily specialized Americanists will learn much from his excellent bibliographies, as well as from the carefully selected myths themselves. The only major gap in his knowledge is in the North Pacific area, Salish especially, and this is almost wholly traceable to his failure to consult the ample collections of James Teit and the Jesup Expedition—sizable folios which have a way of getting shelved in the wrong places in the best libraries. Thompson thus provides us with examples from peoples unrepresented in Pettazzoni: Tahltan, Lassik, Thompson Salish, Lillooet, Nez Percé, Quinault, Okanagon, and Shuswap. There is also no indication of classification of the tales selected by the use of Aarne-Thompson *Märchentypus* and Thompson *motif* numbers, and one devoutly hopes that these will be supplied in the index volume. These lacunae in Pettazzoni are the more surprising because of his otherwise thorough coverage, and they are the only serious blemishes on a work well-conceived and well-implemented, well-printed with the minimum of typesetter's agonies

in the presence of barbaric Indian and English proper names, well-illustrated with maps and colored plates, and well-designed for eye and hand to grasp.

FRANCIS LEE UTLEY, *The Ohio State University*

The Sinhalese Folk Play and the Modern Stage. E. R. SARATHCHANDRA. Colombo: Ceylon University Press Board, 1953. v, 139 pp., 27 plates. Rupees 15.

Sarathchandra's scholarly treatment of the development and present expression of the various types of Sinhalese folk plays is in several respects a work of anthropological significance. His descriptive accounts of the folk dramas, based on direct observation as well as on earlier accounts, have been placed skillfully in the historic and cultural contexts of Buddhism, folk supernaturalism, and diverse Indian influences. Observations on the "modern" theater will be of particular interest to the student of acculturation. Inferences regarding the development and significance of various plays and play elements are cautiously drawn and are based upon objective scholarship in Indian and Sinhalese folklore, and linguistics.

Beginning with "The Cultural Background," there is a concise and excellent statement of the integrated separateness of folk supernaturalism and Theravāda Buddhism, and the role of each in the stimulation of the arts. The play aspects of supernatural rites are treated, and the labyrinthian complexities of the Sinhalese demon world are given what is probably their most intelligible, albeit condensed, exposition available in English. Moving from the dramatic elements in supernatural, especially demonological, ceremonies, Sarathchandra finds roots of the secular masked plays, *Kōlam*, in the deritualization of the demon ceremonies. In addition to demonological rites and the *Kōlam*, the author identifies and describes the *Sokari*, also played with masks, the *Nādagam*, folk musicals or "operas" of fairly recent origin and often of Roman Catholic inspiration, the puppet play, and the Roman Catholic musical passion play.

The modern theater (in distinction from folk) originated in a hybrid form (Nurtiya) partially rooted in the *Nādagam* but more directly an outgrowth of North Indian, especially Parsee, influence in stagecraft and singing, while frequently utilizing European plots. Apart from its establishment of a body of music for the enjoyment of a people losing their musical traditions, the *Nurtiya* was to have no lasting place in Sinhalese culture. With its recent demise, a modification has achieved some current success in stage, and more recently cinema, form.

Contemporary developments in the urban Sinhalese theater are given scant treatment, but it is evident that no lasting indigenous play form has resulted from the cultural hodge-podge of the *Nurtiya*. Apparently Sinhalese drama has a future in so far as its village forms can be preserved and an urban stimulus can be derived directly from a vigorous English language theater group.

BRYCE RYAN, *Wayne University*

PSYCHOETHNOGRAPHY

Character and Social Structure: The Psychology of Social Institutions. HANS GERTH and C. WRIGHT MILLS. New York: Harcourt, Brace and Company, 1953.

This book is no less than an attempt to lay down a social-psychological framework for the comparative study of world society in all its historical and contemporary variation. The task to which the authors address themselves is one of formulating a model which will permit the comprehension and explanation of the individual in society. Part

I is a presentation of models of character structure and social structure. Parts II and III, the major portions of the book, are devoted to a progressive elaboration of the components and interrelations of these models with reference to specific empirical data drawn from a wide range of contemporary and historical societies. Hence, the model emerges from and is relevant to empirical data.

The key to the model of character structure lies in the psychology of George Mead and Freud. It is a psychology in which personal consciousness and the self-image are anchored in the social process, and it emphasizes the plasticity of character structure according to social context. Mead and Freud meet in their conceptions of the generalized other and the superego. But, where Mead's generalized other incorporated the "whole society" and Freud's superego a limited set of kin, for Gerth and Mills the generalized other is formed by those who are "authoritatively significant" in the career pattern of the person. Hence, "conscience" can change over time in phase with changes in career patterns. The generalized other is socially relative within societies and, in keeping with the findings of anthropologists, between societies. Freudian psychology is loosened from its organic determinants and the family context. Feelings, sensations and impulses, in the authors' terminology, are made psychologically relevant in so far as they are socially transformed into emotions, perception, and purpose. These make up the psychic component of character structure, and in this way they employ the Freudian mechanisms of anxiety, hostility, and aggression as these are expressed in role behavior.

The concept of role occupies a central place in the model and serves as the link between character and social structure. The person, which together with the psychic structure and the organism comprise character, is "composed of the combination of roles which he enacts." Motives are learned in response to the expectations placed upon the person by others and are "internalized from the roles which persons enact." The dynamics of the social formation of the person (the place of language, self-image, unities of self, generalized others) are more fully and systematically developed than the concept of role in relation to institutional behavior. The discussion of roles occurs under these headings: The Institutional Selection of Persons, The Institutional Formation of Persons, The Theory of Premiums and Traits of Character. Although not explicitly stated, the constituent elements of roles consist of necessary information and sets of skills, attitudes, loyalties, and values. Adequate role performance is assured through the authoritative use of sanctions within the institutional setting.

An organization of roles makes up an institution, and the specific content and arrangement of roles within an institution is guaranteed by authority (e.g., parental authority guarantees the role of the child). Authority takes on a central place in the analysis of specific institutions, and the authoritative heads of institutions comprise an important segment of the psychologically significant "others" in the experience of the person and so shape his character structure.

Institutions are selected as significant for study according to the ends which they serve—power, goods and services, violence, deities and procreation. Hence, the major institutional orders, so named, are the political, economic, military, kinship, and religious. Each institutional order may encompass the "spheres" of education, status, symbols, and technology. It is recognized that this specification of ends and institutions, the statement of which in itself is a reflection of Western thought and social structure, is not common to all societies. The model serves the purpose of sharpening the observer's perspective as it encounters specific cases. While developing the model, the authors apply it to a vast range of empirical data drawn from ancient China, Rome,

modern Russia, Japan, Latin America, and, particularly, the United States and Europe. This, in itself, is an impressive display of learning, relatively rare in contemporary social-science writing, and matches the scope of a fading era of anthropological greats.

The discussion of "The Unity of Social Structures" is of special interest to anthropologists who are concerned with comparative analysis. As an alternative to the *geist*, themal, logicomeaningful and rural-urban models which attempt to treat "wholes" as a single unit, a model based on units appropriate to specific social structures and cross-comparisons between them is offered. The approach is illustrated with short case studies of Sparta and Rome.

In Part IV, called Dynamics, the system of explanation is applied to three problems regarded as central to modern sociology (chapters on Social-Historical Change, The Sociology of Leadership and Collective Behavior). The closing chapter, Master Trends, must be regarded as a case study application of those elements in the framework which are relevant to an analysis of macroscopic trends in contemporary Western society. Here, turning to the world as it is, the focus is on the co-ordination of political, economic, and military orders, the bureaucratization of major segments of modern life experience, the decline of liberalism, and the shaping of character structure in a polarized world.

It is difficult to characterize the book as a whole. It is not a textbook in the traditional sense (though it may easily be used as one at any level), because it does not manifest that tendency often found in texts to classify and illustrate "all concepts in the field." The authors have drawn on the best literature available in modern social science (ranging from Burckhardt to Troeltsch, Kroeber to Kardiner, and Veblen to Weber, whose work is heavily relied upon throughout) and cope with most of its central problems. They have succeeded in systematizing a substantial portion of the field without creating a closed and rigid system incapable of accommodating to the facts of life and without isolating the individual and his mind from the social and historical setting. Perhaps it is best to say that they provide a systematic perspective within which observable reality in its variation and complexity can be comprehended.

The book has the uncommon virtue of being written in excellent literary style, marred only by the superimposition of unnecessary tables which do not enhance the already logical presentation. It is highly recommended to the anthropological audience as central to the field.

ARTHUR J. VIDICH, *Universidad de Puerto Rico (Rio Piedras)*

Themes in French Culture: A Preface to a Study of French Community. RHODA MÉTRAUX, MARGARET MEAD, and others. Stanford: Stanford University Press, 1954. xi, 120 pp. $1.50.

As the format and typography would seem to indicate, this is an experimental book. For one thing, it applies anthropological techniques to the study of a highly complex, literate culture. The method is not a new one; but this is the first time, to my knowledge, that it has been systematically applied to France. For another, the researchers deliberately refrained from studying French culture in the flesh, as it were. Only informants residing in America and materials available here were used. The intention is to compare the results later on with data obtained in field surveys; it is hoped that the comparison will throw light on the effectiveness of such studies-at-a-distance, which are indispensable in dealing with inaccessible cultures like those behind the iron curtain.

The result is an interesting piece of work. It is not always well written; it succumbs to jargon; but it contains some fascinating and even entertaining passages which are sometimes of more than anthropological significance. The American parent, for example, who has difficulty persuading his child to finish his plate might try the French method: give the child the leftovers for his next meal; this not only works but is far more economical than the approved American technique in such a situation. On the whole, the book is fun to read, if only because of its subject.

The analysis focuses around one structure which the research group felt was central to French social life: the *foyer*. The word is not easily translatable. Sometimes it refers to the household, sometimes to its members; in composition the *foyer* is comparable to the nuclear family: married parents and unmarried children.

The themes treated are dealt with in relation to this structure: the French stress on privacy and the closed world of the *foyer;* the importance of control, and education in the *foyer;* fear of the dangers of the external world, and preparation for these dangers within the *foyer;* and so on. These "thematic regularities," derived primarily from conversations with informants on all manner of problems connected with the *foyer*—father-mother relationships, father-daughter, mother-daughter, etc.—have been checked against the responses of French subjects to projective tests, plot characteristics of French films, and some printed materials, particularly the French *Code Civil*—discussions of which are appended in the form of "Three Background Papers."

The book is necessarily uneven. The best sections are those dealing with the personal interviews; these contain some important insights into French social attitudes, often expressed with a vividness that only direct testimony can convey: "A family of five children," said one informant, "that's not intelligent. That is a manifestation of animality, of backwardness. Everything must be governed (*dirigé* [I should prefer "under control"]); it is only that way that something good can be achieved. . . . " The weakest is probably the chapter on "Plot and Character in Selected French Films," which reaches its nadir in using a film called *Circonstances atténuantes* to prove something about French culture (p. 101); unfortunately, the plot of this particular movie is one of those staple items freely shared among the film industries of various countries which prove only that people will go to the cinema simply to go. Hollywood had its own version with Edward Everett Horton. How many other films of those cited by Miss Wolfenstein and Mr. Leites fall into this category is hard to say; in any event, the student of French society will treat this kind of evidence with great caution.

On the whole, the book, with its faults and virtues, whets the appetite. One can only hope that the Columbia University research group will soon settle down to a detailed and systematic investigation in France. For one thing, this will lead, I think, to certain important adjustments in emphasis. The *foyer*, for example, significant as it is as the basic unit of French social life—as the nuclear family is here—will no longer be treated as though it existed in some sort of splendid isolation. Its links with the larger family especially will be explored, and the uniqueness of the French society—those factors which make the *foyer* different from the American home—will come out as they do not in this volume. At the same time, Métraux and her associates will be able to study the regional and class differences in behavior which they rightly feel are so significant. It is this study on the spot which will, in this writer's opinion, yield the meaningful data. The present analysis at a distance is not the sort of thing I should like to see used as a basis for American judgments of, or policy on, France; fortunately, the authors make no effort here to draw from their "thematic regularities" any conclusions as to French social and political behavior.

One last point. Any research into social attitudes or cultural "themes" must necessarily deal with matters intrinsically vague and imprecise. Extreme caution must be used in screening and evaluating the evidence. Above all, the researcher must take care not to take his hypotheses as facts without actual proof. In the chapter on French projective tests, we are told (p. 110) that the invocation by French parents of the bogey-man *may* be a measure to prohibit the child's nocturnal curiosity; on the next page, a new paragraph begins: "The French prohibition against the child's nocturnal curiosity seems to have the effect of stimulating an intense wish to see everything clearly and distinctly." That type of reasoning can lead to great heights of elaboration; but proof will have long since been left behind. It is hoped that the more substantial evidence available—and long neglected—in France itself will render such ingenuity unnecessary.

DAVID S. LANDES, *Columbia University*

Pygmies and Dream Giants. KILTON STEWART. New York. W. W. Norton & Company, Inc., 1954. 295 pp. $3.75.

This is a popular account of the author's work and life among some tribes of north central Luzon—the Negritos of Bataan and the Zambales Mountains, the Ilongots, Ifugao, Bontoc, Kankanai (Igorots), and Kalingas—more than twenty years ago. The scientific aim of his year's travels was to determine "what kind of native intelligence the people of these nonliterate societies had in the beginning, and then to find out what kind of mind was built up on that basic intelligence by the group processes in which they found themselves involved, especially as revealed by their dreams, visions, and ceremonial procedures" (p. 25). Once these facts were determined, he could then test his "favorite psychological theory—that all men develop from a single mental pattern, and that this pattern can be discovered by a psychological study of successive level of humans, ranging from the primitive to the sophisticate" (p. 16).

Despite this scientific aim, this is not a "scientific" book. It is rather a personal narrative describing the author's experiences, both physical and emotional, in trying to get to, and while living with, the above-named tribes; and the accounts of his scientific investigations are interlaced with the general narrative account. Indeed this book, in one sense, is an emotional autobiography. Its opening chapter recounts a highly emotional experience, intimating personal conflict, and the last chapter ends with an intimation that the author's conflict has been resolved in the course, or by virtue, of his field experiences. In brief, Stewart has done something which, so far as this reviewer knows, is unique. He has attempted to get at the extrascientific motives in his own research and, specifically, to discover his (unconscious) motives for deciding to study and to live with primitive peoples. And this, I think most field workers will agree, is both an important and difficult task.

Stewart attempted to achieve his scientific aim by administering psychological tests (such as the Moral Ideology, Porteus Maze, and the Goodenough-Draw-a-Man), collecting dreams, and observing ceremonies; and by comparing the test results of the groups he visited with one another and with those of subjects he had previously tested in the United States and in Hawaii. The test results, so far as Stewart reports them, seem to confirm his hypothesis: children in all these groups reveal highly similar characteristics, but there are notable differences among the groups as far as adults are concerned. For example, "On all the tests," Negrito children of ages four through six "made scores which did not differentiate them at all from the American children, or from the children of the other five language groups I had tested in Hawaii" (p. 61). Again, "Judging from the test results [of Ifugao children] I might as well have been testing in an

American or Hawaiian school. In all of them the children scored near the American norms" (p. 207). On the other hand, the Negrito adults, for example, made poor scores on those tests that demanded abstract thought because their "experience or education . . . [had] locked up, rather than developed, their capacity to do abstract, reflective thinking" (p. 62).

These statements, if true, are of considerable theoretical significance. This reviewer, however, is somewhat reluctant to accept them—particularly those concerning the children—at their face-value, and not because of a traditional anthropological bias against cross-cultural generalizations. First, the raw data are not given, and there are few tests whose results are not amenable to differing and sometimes conflicting interpretations. Second, the protocols of the Emotional Response Test, which Stewart considered to be "the heart of my whole program," were unfortunately lost when his raft capsized in the river, so that generalizations concerning the results of this test had to be based on "compilations" he had previously made in his notebooks.

Stewart's most interesting findings, however, are not concerned with tests, but with dreams and psychotherapy among the Negritos. Apparently there is some notion among these people of a psychogenic basis for certain kinds of physical illness, and a consequent belief that these must be cured in a special way. With the group assembled, the shaman puts the patient into a trance, in which state the latter encounters and, if he is to get well, "conquers" the spirit that reputedly has caused the illness. This is accomplished by inducing the patient to return in fantasy to that point in his life in which he first became aware of his illness (when the spirit first entered him), to recall all the events surrounding and immediately preceding this point, and to relive them, as it were. In so doing he is eventually led by the shaman to those specific events in his past which were responsible for that emotional conflict which is the basis for his somatic complaints. By living through those terribly painful (and therefore repressed) events, he can conquer the spirit and compel it to become his "helper." When this happens he is cured of his illness, and he becomes a shaman himself. The parallelism between this therapeutic technique and that of modern dynamic psychiatry is too obvious to require explication.

But this is not all. While in the trance state the patient is instructed by the shaman to bring back some artistic creation from his spirit helper—a poem, a song, a dance—and he does! Stewart suggests that this practice sheds some light on the old problem of the relationship between neurosis and creativity. For when the shamans requested "that the area of the personality which had formerly expressed itself as conflict, rage, and migraine headache change itself into music, poetry, and dancing on the spot, . . . they were requesting that the subject transform the pain into that which was socially significant and beautiful" (p. 47).

Although the images which the Negritos have in their trance states are viewed by Stewart as having positive (therapeutic) consequences, he views the images which they have in their dreams ("dream giants") as having negative consequences. The dreamer perceives these images to be as objectively "real" as the percepts he has in the waking state, and this confusion of fantasy and reality, Stewart believes, "partly explained why the Negrito did not grow up mentally and learn to think in abstractions, why he could not change his environment or his social order" (p. 68). For a change in either, he argues, would mean that the contours of the Negrito's external world would no longer correspond with those of his inner, dream world.

The latter interpretation is typical of the many insights that are liberally scattered throughout this book. Like many insights they are valuable if they can be stated in

the form of testable hypotheses. But this is a "popular" book, and it shares at least one of the characteristics of its genre—many of its speculations are stated as fact rather than as hypothesis. This is particularly true in the last two chapters, which are devoted almost exclusively to Stewart's reflections on the nature and condition of man. Some of these reflections are not only dogmatically stated, but their language is obscure to the point of unintelligibility. Stewart's excellent narrative style is not suited for discursive writing. Nevertheless this remains a stimulating and thoroughly enjoyable book.

MELFORD E. SPIRO, *University of Connecticut*

OTHER

World Population and Production: Trends and Outlook. W. S. WOYTINSKY and E. S. WOYTINSKY. New York: Twentieth Century Fund, 1953. lxxii, 1250 pp., 338 figs., 497 tables. $12.00.

Financed jointly by the Rockefeller Foundation and the Twentieth Century Fund, this grandiose work contains 1,250 double-column pages and 31 chapters dealing with Man and His Environment, World Needs and Resources, Agriculture, Energy and Mining, and Manufactures. A second Woytinsky volume, presumably equal in scope and size, is to appear in a year or so and is to deal with progress in world trade, transportation, and government.

The purpose of the first volume is "to outline world economic forces and trends during the fateful era in which mechanized economy . . . is becoming the universal civilization of mankind," the aim being to "describe, measure and explain" these forces and trends. The focus is thus on the world as a whole, a study of social and economic change on a grand scale.

One can hardly quarrel with this intention. Studies are needed which embrace the entire world during the last two centuries of rapid change. But precisely because of this broad point of view a successful undertaking would require carefully delimited criteria of relevance so that unity and coherence could be brought to the subject. It would require systematic and subtle interpretation to explain the causal connections between different aspects of social and economic change. This volume, however, falls between two stools. There is some interpretation, but it lacks focus. Also, a multitude of statistics and "facts" are given whether or not they have any relation to theoretical issues. The result is something between a contribution to social science and an encyclopedic compendium.

A booklet accompanying the volume explains that the work is solely that of the two Woytinskys, working without research contributors or collaborators. Presumably this is construed as a virtue, and it would be if the work were analytical in character; but with due respect for the indefatigable energy of Mr. and Mrs. Woytinsky, it should have been obvious that two people alone could not do justice, *at the descriptive level*, to subjects which range in character from the geography of the earth to migration and health, from races of man to languages, religions, atomic energy, and unrendered pork fat.

Illustrating the penchant for irrelevance, let us ask why the authors begin with a 31-page description of the earth. Presumably they may have wanted to explain the maps and regional breakdowns they are to use subsequently in giving world distributions, but this would have taken only a few pages. Instead, they go ahead and try to write an elementary physical geography, including a table listing the highest mountain peaks, another listing the length of the longest rivers, and an account of how deep

the ocean is. If these things are relevant to economic and social change, the relevance is never made clear. Similarly, why do the authors feel called upon to describe the races of mankind? Relying on Haddon of 1925, they distinguish "three main racial groups"—the white (curly-haired), the yellow-brown (straight-haired), and the black (wooly-haired)—and they tell us the exact percentage of mankind found in each of these groups. The whites, however, they say, include people with "Ugro-Finnish blood." Again, why try to classify languages? At no point do I recall a discussion of how linguistic differences affect economic and productive development, and yet the authors feel it necessary to classify languages. They divide them into those spoken by peoples of European stock, those "used in China and India and countries related to them by mores and religion," and others. A table based on this classification is given for the world, but fortunately the world map of languages is a bit more subtle. In any case, the handling of languages is irrelevant to the book's alleged purpose. It comes under the heading simply of miscellaneous misinformation.

The book is written for the general reader rather than the professional social scientist. Most chapters are rather hasty restatements of current knowledge, with masses of data accompanying the text. Necessary qualifications and contrary views are often omitted. For instance, a whole page and a paragraph are given to the shifting "center" of population in the United States, but no mention is made of other methods of computing the center which would put it in a completely different region. We are told that the Mayas had at least four and perhaps six cities with 200,000 or more people, and seventeen to nineteen with 50,000 to 60,000, and that 30 to 40 per cent of the Mayan population lived in cities; but no mention is made of conflicting views among archeologists. One is also surprised to learn that preconquest Mexico City had perhaps 650,000 inhabitants. In many cases the authors get themselves out on a limb needlessly, the subject beyond their competence being something they did not need to deal with anyway. There are, for example, numerous references to "primitive" life. The most interesting occurs on page 265, where it is said that African natives, before the coming of the white man, "had no wants." This Garden of Eden theory of savage life is hardly relevant to the topic of the volume, but it turns up several times, since the authors seem to find it necessary on each subject to deal with "origin and development."

Each specialist will probably find those sections devoted to his own field to be elementary and in some cases unsatisfactory. The chapters on population, for example, read like a condensed textbook, a bit erratic and somewhat out of date. The net and gross reproduction rates are presented as if they had predictive value, and little is said of newer refinements in the measurement of fertility. Crude rates are sometimes compared when they should not be, e.g., the comparison of rural and urban death rates. Some surprising statements are made (that countries which lose population are doomed, that the age at marriage "varies little from country to country"), but for the most part the treatment is simply dull and uninspired.

Since the book is in part a compendium of international statistics, one of the gravest criticisms is that worthless data are often utilized. Completely erroneous birth and death rates, for example, are given for "selected" countries in tables which array the countries by the order of magnitude of the rates. The authors are cavalier about bad statistics. In the Introduction they say, "When a country with rudimentary school and public health services reports a death rate lower than that of the most progressive countries, this rate reflects the inadequacy of the country's statistics rather than the enviable health of its inhabitants. Such death rates, however, belong in the picture of

world health statistics and cannot be denied a place in international surveys." This is a peculiar form of mysticism: somehow out of bad statistics will come accurate knowledge. On page 163 we find Panama's death rate given as 7.1. How does such an absurd figure contribute to knowledge? Are the sponsors of the volume as irresponsible as the authors? There is some excuse in official publications, such as those of the United Nations, in printing the figures as given out by governments. But even these publications usually try to warn the reader against bad data. It is hard to justify the use in a private study of official figures bearing little relation to reality.

If any chapters could be singled out as better than the rest, they would be those on the labor force and those on agriculture. On these topics the authors appear to have some personal competence. On the whole, however, it can be said that this volume represents an extraordinary effort of two people to write an encyclopedia. Yet today the range of social science is so great that a successful encyclopedia can be written only by a body of experts.

KINGSLEY DAVIS, *Columbia University*

Individualism Reconsidered and Other Essays. DAVID RIESMAN. Glencoe, Illinois: The Free Press, 1954. 529 pp. $6.00.

This book is of value to anthropologists for the following reasons. (1) It presents a study of individualism and of culture in the United States by a keen observer who is learned, capable of a high degree of synthesis, and often original. (2) Although it presents a considerable number of stimulating and provocative ideas concerning the behavior of American man, many of these ideas are more the result of brilliant insight than of controlled methodology. His data are often more illustrative than confirmatory. Anthropologists might well want to engage in testing some of his suggestions about man in American and other cultures. (3) It presents an analysis of what Riesman considers mainsprings of behavior in the United States. Since American anthropologists are a part of the society carrying American culture, it is probable that the course of American anthropology in the past few decades has been influenced by developments that Riesman interprets.

The book consists of a selection of Riesman's essays published in some fifteen journals and half a dozen symposium volumes between 1947 and 1953. Integrating comments are added. Some of the essays are revised more than others. The major sections of the book are: I. Introduction; II. Individualism and Its Context; III. Marginality, Minorities, and Freedom; IV. Culture: Popular and Unpopular; V. Veblen and the Business Culture; VI. Freud and Psychoanalysis; VII. Totalitarianism; and VIII. Problems of Method in the Social Sciences. Obviously, some of the sections deal directly with individualism while others are more tangential. Obviously, also, a review of this length will leave out much that might be said about the book.

Riesman was trained in law and taught that subject before he turned to and taught social science at the University of Chicago and elsewhere. He uses data and approaches from many disciplines. A rough count indicates that in this book he made use of ideas or material from some fifteen different people usually considered anthropologists, not including such individuals as Durkheim, Freud, the Lynds, and Sumner, who sometimes use anthropological approaches. Essay titles such as "Football in America: A Study in Culture Diffusion" and "Toward an Anthropological Science of Law and the Legal Profession" have obvious anthropological appeal. This is true also of many essays not so obviously titled.

Among Riesman's concepts are the "nerve of failure" (the courage to accept the

possibility of failure without being morally crushed), "other-directed" (people dependent upon external authorities), "inner-directed" (people dependent upon an internalized authority), and "autonomous" (people capable of conscious self-direction). It is the autonomous type of character structure that enables a person to tolerate freedom and thrive on it. However, each character type has its virtues and vices. "The inner-directed person tends to be rigid and intolerant of others; the other-directed person, in turn, is likely to be flexible and sensitive to others" (p. 113). "An autonomous person has no compulsive need to follow the other-direction of his culture and milieu—and no compulsive need to flout it, either" (p. 116), but he often feels isolated. Apparently no society is composed entirely of any one character type. In any particular society at a particular period of time the tradition-directed type, the inner-directed type, or the other-directed type may be characteristic because of the cultural conditions prevailing. The autonomous type may exist in any of the three situations.

American culture of the past twenty-five years is conducive, at least in the urban middle class, to producing other-directedness. Such other-directed people are sensitive to and need the approval of others, strive not for internalized ideals so much as for the best possible solution to a particular situation, and feel helpless and anxious. Therefore they do not resist authority or exercise freedom of choice even when they may do so. Their feeling of helplessness is influenced by a change in attitude toward work. Work in the past "was considered a mode of relating oneself to physical objects, and only indirectly to people" (p. 109). The modern worker, no longer producing finished products and demonstrating technical skills in which he takes pride, is not engaged in a struggle for mastery over himself and nature but with adjusting to fellow-workers and management. However, American culture is rich and varied. Although we develop in American society, we may transcend the narrowness of group-defined emphases with individual breadth.

Riesman's book presents not a formalized approach so much as a many-faceted way of looking at everything. Anthropologists reading it will doubtless frequently depart from what he is saying for mental excursions of their own. For instance, Riesman on page 423 approves "a pluralistic, somewhat disorderly, and highly competitive society" and mentions that social science is concerned with "categorizing human beings and social systems." Would it not be worthwhile for us to re-examine the material on functionalism to see how much of it was written with the ideal of an orderly, single-focused society as a touchstone? And if some was so written, might it fruitfully be reconsidered from a different approach? The same applies to personality studies. How often has a unidirectional personality been considered the ideal and the standard for evaluation?

Whether Americans of the urban middle class are now tending to be "other-directed" or not has not been conclusively proved. Probably they are. Even more probably, it seems to me, they are becoming other-oriented. That is, despite our technological successes, many are becoming more interested in a world of people rather than in a world of artifacts and nature. When they use artifacts, they often use them to impress people. The frontier to be conquered is not so much one of things as one of people, and this helps direct attention to them. However, although emphases differ, culture in all parts of the world involves not only patterned relationships of people to people and groups to groups but also patterned relationships of people to the material things they make, to their natural environment, to their supernaturals, and to their symbol systems. In much of recent ethnology there has been a declining activity in material culture studies and an increased activity involving social structure and per-

sonality in culture. This has produced many typological concepts for both social structure and personality.

If human behavior is patterned and if it consists of relationships to manmade and natural things as well as to people, should we not be able to develop typologies and classifications for these relationships? If we are going to do cross-cultural analyses, should we not have classificatory concepts for all categories of culture? For some time, this reviewer has been trying to develop either structural-functional or psychological typologies for use in cross-cultural analyses of man's relationships to the tangible things he has produced. Much of the anthropological literature is not helpful.

Perhaps we should ask ourselves if American anthropologists, as members of American culture, have been influenced by other-orientation. Maybe the rise in popularity of studies of social structure and of personality, extremely worthwhile though they are, reflects recent American culture. Maybe to be individualists and also to be thorough scientists we need to do more thinking about other categories of culture. Certainly we must be careful not to neglect cultural categories simply because they are not currently emphasized in American culture.

Riesman's book, through defining more clearly both American culture and the possibilities for individualism in it, can aid anthropologists. It is not always easy reading. His sentences are often long; his ideas are often complex; but his contribution is a real one. The greater our knowledge of implicit and explicit aspects of American culture, the greater the probability we can guard against biases of American culture and Western culture when we study other cultures. The book is recommended.

HARVEY C. MOORE, *The American University*

The Savages of America: A Study of the Indian and the Idea of Civilization. ROY HARVEY PEARCE. Baltimore: The Johns Hopkins Press, 1953. xiii, 252 pp. $4.00.

Pearce's work is not a new ethnographic handbook, although its principal title might erroneously suggest comparison with Wissler's out-of-print work. It is an interesting study of the origins, development, and effects prior to 1850 of a once important belief system within our own culture. The material is rich, and anthropologists should not overlook it.

The belief examined is that of the American Savage, the American Indian as exemplar of savagism, the opponent of civilization and civilized men, and specifically of the European settlers of the eastern North American seaboard. Chronological limits of the study are set at 1609 and 1851, the dates, respectively, of Johnson's *Nova Britannia* and Morgan's *League*. An opening section surveys the development of attitudes toward American Indians in the English-speaking colonies between 1609 and 1777. Two themes run through this colonial record; they might be designated as the Incorrigible Savage and the Nature's Noblemen motifs. In the first, the envisaged role of the Indian vis-à-vis the settler shifts from that of a destitute fellow-being to be civilized to that of an irreclaimable menace to be destroyed. In the second, the Indian comes to be regarded as the possessor of certain virtues, as well as of the vices natural to his savage state, and a paradox is engendered: civilization is good, but it destroys the good of the Indian along with his evil.

The first theme was played through in Virginia in a mere fifteen years (1607–22), and variations in New England, Maryland, Pennsylvania, all ended in bankruptcy of the hope, initially expressed everywhere, of converting the American savage to European civilization. The climate of opinion was of course that of the seventeenth

century. The most sympathetic expositions of the Indian's state could not credit these people with value systems of their own which might resist cultural assimilation.

Robertson's *History of America*, appearing in 1777, and the works of other contemporary Scottish writers crystallized the notion of progress and fitted the American Indian into relation to it. Pearce discusses in detail these and other late eighteenth-century accounts in which the idea of Indian savagism takes the form it shows throughout the first half of the nineteenth century; they add up to an evaluation of an extra-European society as inferior and unworthy of survival. The Indian's old order had to give way to a better and higher new order, and the idea of conversion or sharing of civilization faded in importance.

Late eighteenth- and early nineteenth-century factual studies of Indian life are also surveyed. These were characterized by the fitting of facts to the idea and by failure to separate description from evaluation. A prime example is Heckenwelder's account of the Delawares (1818), and the whole group of studies culminates in the work of Schoolcraft. These works remain fascinating examples of the pressure of a system of belief and interpretation on a set of avowedly objective studies.

Three chapters describe the Noble Savage tradition as applied to American aborigines between the Revolution and the 1850's. Pearce shows a long American development of primitivism, the cult of the nobility of natural man, in part quite different from and independent of the ideas stemming from Rousseau. Chapter 6, on stereotypes of the Indian in dramatic and poetic literature of the period, is recommended to anyone tempted to regard modern entertainment media as setting an all-time record of silliness. The asininity of these productions is perhaps less significant than their solemnity and the seriousness with which they were received. Fictional treatment in the same years finally argued the conflict of feeling over the Noble American Savage out of existence, according to Pearce, by showing that his nobility was one with his ignobility. His nobility was a special savage nobility, destined to disappear with the inevitable eradication of savagery from this continent. Cooper focused the tradition, but was preceded and followed by many others who rang the changes on the mingled admirable and hateful qualities of the Redskin.

A final brief consideration of treatment of the Indian since the middle of the nineteenth century ends with the somber conclusion that the effect of all this on American culture may be permanent. "Our civilization, in subduing the Indian, killed its own creature, the savage. . . . It has not been able entirely to kill the Indian, but having subdued him no longer needs or cares to. Still, it might be that there will always be somebody who needs to be subdued, men to . . . be savages standing in the way of civilization" (pp. 243–44).

There are flaws in this work. The author's phrasings are here and there a little vaporous. There is a bit too much emphasis on "problems" which were perhaps always literary ones. Pearce's analysis in terms of Idea, Image, Symbol seems at times forced on the material rather than demanded by it. However, the anthropological reader will not meet with any really unintelligible jargon such as may occasionally confront him in the works of professional colleagues, and he will probably find the book worth his attention.

WILLIAM W. ELMENDORF, *University of Washington*

Clasificación Decimal de la Bibliografía Antropológica: Proyecto de Extensión. VICTOR A. LITTER. Buenos Aires: I.D.E.A., 1953. 44 pp.

An Argentine anthropologist-librarian submits his proposed expansion for Anthro-

pology of the Universal Decimal (or Brussels) Classification. The Brussels Classification is in turn an expansion of the Dewey Decimal system, incorporating, for example, Rudolf Martin's schedule for Somatology and Morphology. Victor Litter, having studied all available classifications from Galton to Murdock, is convinced that anthropologists and librarians would do well to practice intellectual co-operation by expanding the Universal Decimal Classification and adopting it for the arrangement of all their books and bibliographies. To this end he has done three things: (1) provided for the attraction to Anthropology (571/573) of material usually scattered throughout the entire classification schedule; (2) added many new numbers—especially for Anthropological Theory, and Race; (3) developed the number 573 for Ethnology, making use of the first Spanish translation of the *Outline of Cultural Materials*, published by the University of Tucumán in 1939.

Although Litter proposes his scheme as a library classification, and cites numerous books as examples, the Universal Decimal Classification is awkward to use in libraries because of its complicated notation. Not only can the numbers be lengthy—Mead's *Coming of Age in Samoa* is given the number 573.501.016.47.019.745(961.3):159.922.4, while Montell's *Dress and Ornaments in Ancient Peru* would be 573.21(=982.1):571.76/77(85):985.01—but various symbols are employed to express the interrelation of subjects. Even the United Nations Library in New York was forced to abandon the full use of this scheme because it was so difficult to keep the books in correct order on the shelves. Most of our large research libraries are already committed to the Dewey or Library of Congress classifications which, while they tend to scatter anthropological material, still have to take care of all types of books, to be used from all points of view. The smaller special collections of books acquired primarily for the use of anthropologists will not need such an elaborate classification scheme—in fact will probably be much easier to use if they are arranged under a few broad categories.

Obviously there is considerable interest in the organization of anthropological knowledge—witness the new Spanish translation of the *Outline* published by the Pan American Union (*Guia para la Clasificación de los Datos Culturales*) and manuals for field work, such as that compiled this past summer under the direction of J.W.M. Whiting for a cross-cultural survey of childrearing by teams from Cornell, Harvard, and Yale. Whether archeologists, physical anthropologists, and cultural or social anthropologists throughout the world will find it feasible to utilize one common numerical scheme, such as Litter's expansion of the Universal Decimal Classification, remains to be seen.

MARGARET CURRIER, *Harvard University*

Community Power Structure: A Study of Decision Makers. FLOYD HUNTER. Chapel Hill: The University of North Carolina Press, 1953. 297 pp. $5.00.

This is a pilot sociological study into an area of social life which has been little explored in most cultures. The approach could be useful for certain types of anthropological studies.

The focus of the book is on the character of social power and on how that power is distributed and used within the context of local social arrangements. Specifically, the author centers attention on the roles of influential persons in determining the process of decision-making among the members of a community, the forms of behavior commonly exhibited by influentials with respect to their own inner circles and to other segments of the local group, the degree to which influence is decisive in governing the actions of the community, and the relationship of social power to the total social structure.

The approach seems to this reviewer to be relevant for those investigating social organization and also for those concerned with the process of social change in various cultures. For the former it offers another dimension by directing attention to the nature of leadership in local groups and to the patterns of social control, both of which are sufficiently marginal in other schemes as to be seldom comprehensively surveyed in field work. For those concerned with social change, it serves as a needed supplement to the cultural and psychological frames by giving more primacy to the place of power in the adoption, modification, or rejection of social practices.

Hunter's field work was done in a middle-sized community situated in the southeastern section of the United States. Through interviews with a series of knowledgeable informants, the author gathered social facts that enabled him to delineate the individuals with power in various strata and institutions. The interrelationships between these persons of power are then diagrammed in order to reveal the social matrices.

Similar studies of power structures in other societies would fill a gap in the existing knowledge of a strategic area of social life.

JOHN USEEM, *Michigan State College*

Beating the Tupan in the Central Balkans. YURY ARBATSKY. Chicago: The Newberry Library, 1953. vii, 64 pp., 2 figs., 1 map.

It is unusual for a musicologist to have the opportunity to participate, at any considerable depth, in the music of a cultural tradition other than his own. Arbatsky studied the intricacies of tupan beating (the tupan is a large double-headed drum, struck on the right side with a drumstick and on the left with a switch) during 1933–37 under the tutelage of the Albanian master-tupanist, Mehmed of Špinadija, near Prizren. Of particular interest are (1) Arbatsky's discussion of asymmetrical time and thesy stematic exposition afforded by his extensive notations, and (2) the complex shading of sound made possible by the different combinations of tonal effect from the switch and the drumstick. The evidence for an Arabic origin of tupan technique is explored and Arbatsky goes so far as to suggest that this technique is the key to an understanding of the music of western Asia and India.

DAVID P. MCALLESTER, *Wesleyan University*

Index to the Proceedings (1–13) and to the Journal (1–40) of the Washington Academy of Sciences. MARY A. BRADLEY (compiler). Washington, D. C., 1953. vi. 268 pp. $7.50.

During the forty-odd years of its existence the *Journal of the Washington Academy of Sciences* has carried a good bit of anthropology—physical, archeology, and ethnology principally—which in some years has filled one quarter of its pages. Anthropologists may be gratified to learn that there is now an index of the author and title variety in which they can readily locate all the titles by a given author, or the papers on given subjects, and places. Besides anthropology the index contains a vast deal of biology, geology, and some physics and engineering. Nearly seventy anthropologists, many of whom have published a number of papers in the series, are represented in the index. With an index the *JWAS* assumes new importance as a place of prior publication.

WILLIAM N. FENTON, *National Research Council (Washington, D. C.)*

The Hand-Produced Book. DAVID DIRINGER. New York: Philosophical Library, 1953. xii, 603 pp., 185 illus. $15.00.

This is a companion volume to the same author's *The Alphabet, A Key to the History*

of Mankind (1948, reprinted 1949 and 1952) and is evidently to be followed by a volume on *Illumination and Binding*. It is extensively cross-referenced to the first volume of the trilogy, thus accounting for the lack of specific references on each page, though extensive bibliographies are given at the end of each chapter. The volume is essentially an encyclopedic compilation of information on the hand-produced book all over the world and throughout history. The many illustrations serve as a convenient source for examples of a wide variety of writing systems, writing materials, and types of hand-produced books. (The cuneiform texts in Fig. III-2a and Fig. III-10 [left] are printed upside down.) An index at the end of the book makes the extensive collection of material much more accessible. The volume would seem to serve best as a convenient starting point for investigation of paleographical matters involving most of the literate cultures of the world.

HERBERT H. PAPER, *University of Michigan*

Psychosis and Civilization: Two Studies in the Frequency of Mental Illness. HERBERT GOLDHAMER and ANDREW MARSHALL. Glencoe, Ill.: The Free Press, 1953. 226 pp., 14 tables. $4.00.

There have been a large number of studies of social structure that have used personality data as indices of the operation of social organizations. Durkheim's *Suicide* is, of course, the classic. The present work is in the same tradition. Sponsored by The RAND Corporation, it first appeared in that group's monograph series in July of 1949. In a most imaginative and careful use of existing data, it tries to determine whether there has been any significant change in the rate of serious mental illness in the United States between 1840 and 1940. Faithful readers of other trend studies in this field will now predict that this new research will produce another long, ambiguous answer suggesting that such questions are better left unasked given the difficulties of using old, fragmentary, and unstandardized records. Those readers will be wrong. Goldhamer and Marshall confidently decide that the data are adequate to the task; that no significant changes have occurred.

The heart of the data consists of fairly complete official records for public and private mental hospitals, and other institutions in which mental patients were placed, in Massachusetts. Using these data and state and federal census reports, age-specific rates for first admissions were computed. Less complete data for the nineteenth century and the early part of the twentieth were available from Oneida County (Rome and Utica) in New York, New York City, Fayette County (Lexington), Kentucky, and Davidson County (Nashville) in Tennessee. Despite the expectations we might have that growing recognition of mental disease, growing acceptance of the value of treatment in mental hospitals, and growing availability of such facilities would, in themselves, result in an increased rate of first admissions, no such trend occurs.

The authors then examine the argument that the earlier data are inflated by the inclusion of mental defectives or of neurotics or of epileptics and alcoholics without psychosis—diagnoses that would not be grouped with figures for psychotics in more recent tabulations. From a careful examination of the nineteenth-century diagnostic classifications, they conclude that these distinctions were made about as well then as today. They do not, of course, have material to establish the relative frequency with which the typical diagnostician then and now actually made the distinctions known to the leaders in his field.

There are some technical problems concerning the validity of their rates for the oldest age groups, but, in general, the findings are impressively consistent. The problem

is how they shall be interpreted. The authors themselves suggest that theories stressing early, intimate, interpersonal relations rather than shifts in the structure of the total society, as the seed-beds of psychoses, are consistent with their results. This rests on the assumption that the crucial aspects of such relations are more stable than the overall social structure. This may well be true. So may some other interpretations. It may be, for example, that the constancy of the number of persons known to be psychotic in the sense that they enter public institutions and become a part of official statistics is a function of the constancy of the part of its wealth that the community is willing to spend to support such institutions. In the reviewer's state (Michigan), at least, many mental institutions have had waiting lists, even for patients with rather severe disorders. Again, although changes in the total social structure may have had little effect on rates for psychosis, it may have had considerable effects on the types of psychosis. It would be surprising if the general and continuous rise in the standard of living in the United States and the gradual decline in the proportion of unskilled laborers in the population has not had results for the frequency with which psychoses characteristic of lower-class patients occur in this country. Further, since it is usually found that upper- and middle-class people have lower rates of psychosis than do lower-class people, the constancy of the Goldhamer-Marshall rates may be due to a rise in the rates for upper- and middle-status persons, while the proportion of lower-status persons in the population, with their high rates, has declined.

This book is an important and sophisticated beginning. As its authors conclude, one of its greatest values is that it will shake researchers from their stereotypes and demand the formulation of new hypotheses. It will very likely be a landmark in its field.

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Social Psychology: An Interdisciplinary Approach. HUBERT BONNER. New York: American Book Co., 1953. 439 pp. $4.25.

In this introductory text, not only has Bonner utilized illustrative materials from anthropology, but much of his theoretical content is drawn from that field. This was not generally the case in the various texts reviewed earlier by the present writer (see AMERICAN ANTHROPOLOGIST 55:728–42). As the title indicates, the book includes the work of clinical psychologists, psychoanalysts, social psychologists, sociologists, and anthropologists. Besides two introductory chapters on the development and methods of social psychology, and a concluding chapter entitled, "The Individual in an Unstable World," the material is set in three main categories: "Social Interaction, the social matrix of behavior"; "Culture and Behavior, cultural values and personal-social adjustment"; and "Group Dynamics, social change and collective behavior." The first two are well presented although somewhat eclectic; the last is very weak in view of the extensive work which has been done in the field. The author states in at least two places that the biological individual is important in understanding behavior, but he does not go into the subject at any length.

Bonner's use of an "interdisciplinary approach" has advantages for the beginner in the field of social science. For those who have had introductory courses in anthropology, sociology, or psychology, some of the material will be repetitive. For the person specifically interested in the development of social psychology, the work tends to neglect many of the more recent labors of social psychologists. This is evidently Bonner's intent, since he states: "Social psychology has not been notable for experimental re-

search, and too much of the little that it has accomplished has been disappointingly trivial" (p. 13). While there are certainly some social psychologists who would disagree with this point of view, Bonner's text does provide a well-organized and easily read introduction to the interweaving of the various disciplines which he emphasizes: psychology, sociology, and anthropology.

The references to anthropological works are generally to Malinowski, M. Mead, and Kardiner and Linton. While the works of these writers will remain of first importance for many years to come, the reader might wish here, as in the subjects of group interaction and communication, that he had also resorted to some more recent studies. Since the book is aimed at a beginning audience, the anthropologist will not find it of much use. Had it been issued six or eight years ago, it might have been more significant. As a text it would have been improved had the publishers resisted the attempt to make an original format: the pages are confused with unnecessary indentations and dots.

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The Achievement Motive. DAVID C. MCCLELLAND, JOHN W. ATKINSON, RUSSELL A. CLARK, EDGAR L. LOWELL. New York: Appleton-Century-Crofts, Inc., 1953. xxii, 384 pp., 17 figs., 45 tables. $6.00.

The purpose of this "interim research report," as the authors term it, is to demonstrate the effects of various arousal conditions on imaginative behavior, to show how a measure of achievement motivation is derived from these effects using scoring categories developed for this purpose, and to demonstrate the relation of this measure (*n* Achievement) to various other types of behavior. The present reviewer will not attempt a review of the entire work but will stress portions of interest for certain anthropological problems.

In an attempt to demonstrate that the scoring categories used in defining the achievement motive have cross-cultural applicability, the technique was tested by Lowell with 21 ninth-grade Navaho males in New Mexico. He used written suggestions as verbal cues, rather than culturally modified TAT's. The cues consisted of statements such as these: "A mother and her son—they look worried," "Two men looking for something—one is older," "A boy has just left his house," "A man alone at night," in two forms of four statements each. This innovation in projective techniques elicited responses that could be scored using the standardized achievement motive categories used in the other experiments. Tentative conclusions derived from this experiment are that the scoring categories are sufficiently general for any cultural situation, and that achievement motivation as induced by referring to intelligence, leadership, etc., in the arousal conditions, may be reflected in the same way in fantasy "the world over." The content itself, it is maintained, is unimportant. An American middle-class male adolescent may connect his affect with wanting to be a good businessman, a Navaho with being a good sheepherder, but both achievement motives may be scored using the same general categories, such as "experience of need, or motive," "engaging in instrumental acting," "obstacle," etc.

The second experiment of particular interest to anthropologists tests cross-culturally a hypothesized relationship between achievement imagery and independence training in childhood (the criteria for which are explicated) that the authors have tested in our own society. They rate independence training for eight North American Indian tribes on the basis of the Human Relations Area Files data and relate this to achievement imagery scored for Coyote folktales from the same cultures. It is discovered that both

the age at which independence training is instituted and the severity of independence training correlate very significantly with the *n* Achievement scores derived from folktales.

The raw data for the ninth-grade Navaho boys are not given; so it is difficult to make any very telling criticisms about them. The school-structured situation itself is an important variable, as the authors point out, and as they have been reminded by Kluckhohn and others. Navaho boys, by the time they have been subjected to the exigencies of the white man's school for nine years, have probably learned to give the "right" answers (on the white man's terms) when their achievement motives are aroused by arousal conditions that include comments on being "smart," and being "good story tellers." In some North American Indian groups arousal conditions of this type might have a depressant effect on the expression of the achievement motive, since the "smart" person, who stands out above his fellows in performance, is an object of suspicion, and later—of sorcery. This suggests that in cross-cultural tests using this technique the conditions that arouse the achievement motive will be of as much interest as the intensity or variety of the expressions of the motive itself. A variety of codified techniques of arousal might be used as the projective variable. Coupled with detailed knowledge of the culture, this might prove to be useful data in cross-cultural comparisons.

With respect to the second experiment, objections may be raised concerning the effect of cultural diffusion on the folktales (the authors remark that, "Interestingly enough, the stories from different cultures often had the same themes"! [p. 290]), and concerning the variable affective context of folktales and independence training in different cultures.

But McClelland and his collaborators have done something important in their tentative cross-cultural experiments. They have selected a discrete and seemingly testable relationship between antecedent and consequent variables in cultural determinant and personality formation, they have demonstrated that scoring categories for a projective expression of this possible significant variable can be devised with sufficient generality to permit their use in cross-cultural comparisons, and they have done something that few, if any, anthropologists have thought of doing—they threw away the pictures and substituted verbal stimuli. This avoids laborious cultural modifications of the TAT and appears to work well, at least in the case of the Navaho. In their method lies the possibility of developing relatively tight cross-cultural comparisons.

As a final comment, anthropologists are usually interested in evoking cultural modalities in response. The authors say they are not, because they feel that socially acceptable answers may indicate awareness of what is socially expected, but not necessarily what subjects invest with affect as an achievement motive. In the latter case the investigators seem to be pursuing an unreal possibility, and when they turn to cross-cultural comparisons they slip into an anthropological habit and use a response (that is also a stimulus) that is supremely a cultural modality—the folktale. But anthropologists may indeed ask themselves if the investigators perchance have something in their intention to avoid obviously cultural-cued materials, for anthropologists are sometimes careless about the difference between ideal, agreed-upon culture, and real behavior. The range of affect-involved achievement motivation in a culturally defined group or the range of arousal conditions could be valuable data that might lead the anthropologist to question other aspects of his culture construct, thus making it a better report of behavior.

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